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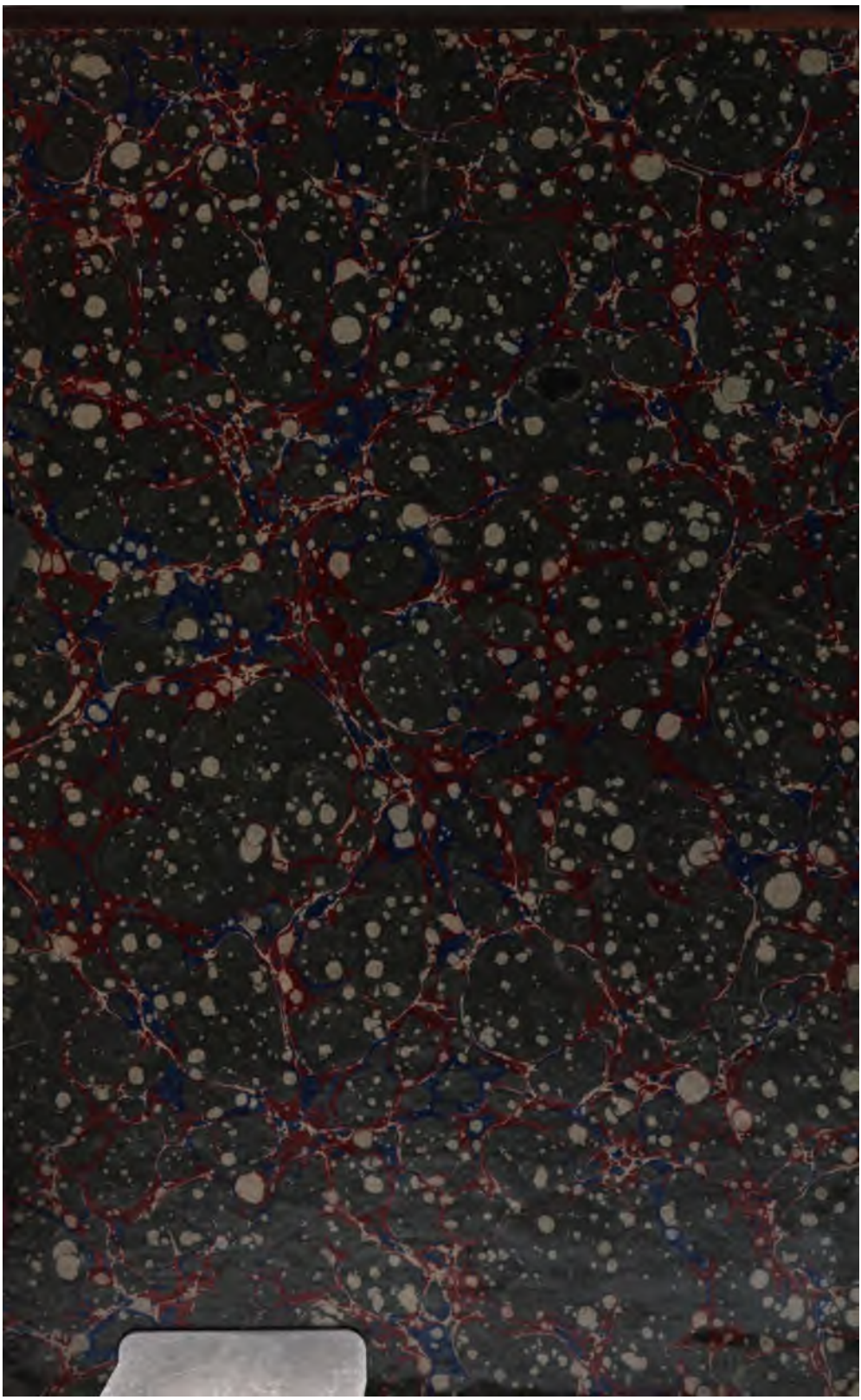
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PROCEEDINGS

OF THE

AMERICAN PHILOSOPHICAL SOCIETY

HELD AT PHILADELPHIA

FOR PROMOTING USEFUL KNOWLEDGE.

VOL. XLII. JANUARY–APRIL, 1903. NO. 172

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PHILADELPHIA:

THE AMERICAN PHILOSOPHICAL SOCIETY,

104 South Fifth Street.

1903.

It is requested that all correspondence be addressed

To the Secretaries of the

AMERICAN PHILOSOPHICAL SOCIETY,

104 South Fifth Street,

Philadelphia, U. S. A.

Members will please communicate to the Secretaries any inaccuracy in name or address as given on the wrapper of this number.

It is requested that the receipt of this number of the Proceedings be acknowledged to the Secretaries.

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PROCEEDINGS

OF THE

AMERICAN PHILOSOPHICAL SOCIETY

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VOL. XLII.

JANUARY TO DECEMBER,

1903.

PHILADELPHIA:
THE AMERICAN PHILOSOPHICAL SOCIETY.
1903.

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HELD AT PHILADELPHIA
FOR PROMOTING USEFUL KNOWLEDGE

VOL. XLII. JANUARY–APRIL, 1903. No. 172.

Stated Meeting, January 2, 1903.

President WISTAR in the Chair.

The list of donations to the Library was laid on the table and thanks were ordered for them from the Chair.

Messrs. Joseph C. Fraley, Patterson DuBois and Harold Goodwin, the Judges of the annual election for Officers and Councillors, reported that the same had been held on this day, between the hours of 2 and 5 in the afternoon, and that the following named persons were elected, according to the laws, regulations and ordinances of the Society, to be the Officers for the ensuing year:

President.

Edgar F. Smith.

Vice-Presidents.

George F. Barker, Samuel P. Langley, William B. Scott.

Secretaries.

I. Minis Hays, Edwin G. Conklin, Morris Jastrow, Jr., Arthur W. Goodspeed.

Treasurer.

Henry LaBarre Jayne.

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Curators.

Charles L. Doolittle, William P. Wilson, Albert H. Smyth.

Councillors to serve for three years.

George R. Morehouse, Patterson DuBois, Ira Remsen, Isaac J. Wistar.

Stated Meeting, January 16, 1903.

President SMITH in the Chair.

The President, on taking the Chair, expressed his thanks for the honor conferred upon him by the Society, and then made some remarks on some recent researches in electro-chemical analysis.

Dr. I. Minis Hays was elected Librarian for the ensuing year.

The Standing Committees for the ensuing year were chosen as follows:

Finance.—Philip C. Garrett, Joel Cook, C. Stuart Patterson.

Publication.—Henry Carey Baird, Joseph Willcox, Amos P. Brown, James W. Holland, Horace Jayne.

Hall.—Harold Goodwin, John Marshall, Frank Miles Day.

Library.—George F. Barker, Albert H. Smyth, J. G. Rosengarten, Edwin G. Conklin, Robert C. H. Brock.

Dr. William W. Keen was elected a Councillor to fill the unexpired term of Prof. Albert H. Smyth, resigned.

Stated Meeting, February 6, 1903.

President SMITH in the Chair.

The list of donations to the Library was laid on the table and thanks were ordered for them from the Chair.

The Secretaries announced the decease of the following members:

Sir George Gabriel Stokes, of Cambridge, Eng., on February 1, 1903, aged 83.

Hon. Fernand Cruz, of Guatemala.

Prof. H. W. Wiley, of Washington, made some remarks on the "Investigations of the Bureau of Chemistry on the Composition and Adulteration of Foods."

Prof. Edgar F. Smith presented a specimen of the metal calcium obtained by electrolysis.

Dr. Samuel G. Dixon made some remarks on the production of tuberculosis by inoculation with tuberculin.

Stated Meeting, February 20, 1903.

President SMITH in the Chair.

A letter was read from Sir Michael Foster, of Cambridge, accepting membership.

The list of donations to the Library was laid on the table and thanks were ordered for them from the Chair.

Dr. A. C. Abbott offered some remarks on "Some of the Problems of the Bacteriologist."

Stated Meeting, March 6, 1903.

President SMITH in the Chair.

A list of the donations to the Library was laid on the table and thanks were ordered for them from the Chair.

The decease of the following members was announced:

James Glaisher, F.R.S., at South Croydon, England, on February 7, 1903, aged 94.

Rear-Admiral William Harkness, U. S. N., at Jersey City, on March 1, 1903, aged 65.

Prof. Angelo Heilprin made some remarks on "The Scientific Aspects of the Pelée Eruptions."

Stated Meeting, March 20, 1903.

President SMITH in the Chair.

A list of the donations to the Library was laid on the table and thanks were ordered for them from the Chair.

The decease was announced of Charles Godfrey Leland, at Florence, Italy, on March 20, 1903, aged 79 years.

Prof. Wilder D. Bancroft, of Cornell University, made some remarks on "The Electrolytic Dissociation Theory, with Application to Medicine and Biology."

General Meeting, April 2, 3 and 4, 1903.

APRIL 2.—MORNING SESSION, 10 A.M.

President SMITH in the Chair.

The President made a brief Address of Welcome.

The following papers were read:

"The Structure of the Corn Grain and Its Relation to Popping," by Prof. Henry Kraemer, of Philadelphia.

"Beaver County (Pa.) Orchids," by Mr. Ira Franklin Mansfield, of Beaver, Pa.

"Development of the English Alphabet," by Prof. Francis A. March, of Easton, Pa.

"Archæology and Mineralogy," by Prof. Paul Haupt, of Baltimore.

"The Activity of Mont Pelée," by Prof. Angelo Heilprin, of Philadelphia.

"The Forward Movement in Plant-Breeding," by Prof. L. H. Bailey, of Ithaca, N. Y.

"Reaction as an Agent in Securing Navigable Depths in River and Harbor Improvements," by Prof. Lewis M. Haupt, of Philadelphia.

AFTERNOON SESSION, 2 P.M.

Vice-President BARKER in the Chair.

The following papers were read:

"The Curtis Steam Turbine," by Mr. W. L. R. Emmet, of Schenectady, N. Y.

"The Principle of Least Work in Mechanics and Its Possible Use in Investigations Regarding the Ether of Space," by Prof. Mansfield Merriman, of Bethlehem, Pa.

"The Nernst Lamp," by Mr. Alexander Jay Wurts, of Pittsburg.

"The Problem of the Trusts," by Mr. C. Stuart Patterson, of Philadelphia.

"On the Dependence of what apparently takes place in Nature, upon what actually occurs in the Universe of Real Existences," by Prof. G. Johnstone Stoney, F.R.S., of London.

EVENING SESSION, 8 P.M.

At the Hall of the Historical Society of Pennsylvania, S. W. Cor. of Locust and Thirteenth Streets.

President SMITH in the Chair.

The following papers were read:

"The President's Address—A Brief History of the Society," by Prof. Edgar F. Smith.

"The Carnegie Institution During the First Year of Its Development," by President Daniel C. Gilman, of Baltimore.

APRIL 3.—MORNING SESSION, 10 A.M.

Vice-President LANGLEY in the Chair.

The following papers were read:

"The Double Star System Σ 518," by Mr. Eric Doolittle, of Philadelphia. Introduced by Prof. M. B. Snyder.

"New Applications of Maclaurin's Series in the Solution of Equations and in the Expansion of Functions," by Prof. P. A. Lambert, of Bethlehem. Introduced by Prof. C. L. Doolittle.

"The Constant of Aberration," by Prof. Charles L. Doolittle, of Philadelphia.

"The Degree of Accuracy of the Newtonian Law of Gravitation," by Prof. Ernest W. Brown, F.R.S., of Haverford, Pa.

"The Mechanical Construction and Use of Logarithms," by Mr. Charles E. Brooks, of Baltimore. Introduced by Prof. George F. Barker.

"The Theory of Assemblages and the Integration of Discontinuous Functions," by Prof. I. J. Schwatt, of Philadelphia. Introduced by Prof. C. L. Doolittle.

"The Franklin Papers in the Library of the American Philosophical Society," by Mr. J. G. Rosengarten, of Philadelphia.

Executive Session, 12.15 P.M.

President Smith in the Chair.

Dr. Hays, by unanimous consent, offered the following preamble and resolution, which were unanimously adopted:

"Inasmuch as the two hundredth anniversary of the birth of Benjamin Franklin occurs in January, 1906, it is proper that the American Philosophical Society, which owes its existence to his initiative and to which he gave many long years of faithful service, should take steps to commemorate the occasion in a manner befitting his eminent services to this Society, to science and to the nation. Therefore be it

"*Resolved*, That the President is authorized and directed to appoint a Committee of such number as he shall deem proper to prepare a plan for the appropriate celebration of the bicentennial of the birth of Franklin, and to report the same to this Society."

The President thereupon appointed the following members to constitute the Committee:

Hon. George F. Edmunds, *Chairman*.
Prof. Alexander Agassiz, Boston.
Prest. James B. Angell, Ann Arbor.
Prof. George F. Barker, Philadelphia.
Prof. A. Graham Bell, Washington.
Mr. Andrew Carnegie, New York.
Prof. C. F. Chandler, New York.
Hon. Grover Cleveland, Princeton.
Prest. Charles W. Eliot, Cambridge.
Prest. Daniel C. Gilman, Baltimore.
Prest. Arthur T. Hadley, New Haven.
Provost C. C. Harrison, Philadelphia.
Hon. John Hay, Washington.
Dr. I. Minis Hays, Philadelphia.
Prof. Samuel P. Langley, Washington.
Capt. Alfred T. Mahan, U.S.N.
Dr. S. Weir Mitchell, Philadelphia.
Prof. Simon Newcomb, Washington.
Governor S. W. Pennypacker, Harrisburg.
Prof. E. C. Pickering, Cambridge.
Prof. Michael I. Pupin, New York.
Prest. Ira Remsen, Baltimore.
Prof. John Trowbridge, Cambridge.
Dr. Charles D. Walcott, Washington.
Hon. Andrew D. White, Ithaca.
Prest. Woodrow Wilson, Princeton.

The pending nominations for membership were read and spoken to. The Society then proceeded to ballot for members.

The tellers subsequently reported the election of the following named candidates as members:

Residents of the United States—

Edward E. Barnard, Sc.D., Williams Bay, Wis.
Carl Barus, Ph.D., Providence, R. I.
Franz Boas, Ph.D., New York.

William W. Campbell, Sc.D., Mt. Hamilton, Cal.
Eric Doolittle, Philadelphia.
Basil Lanneau Gildersleeve, LL.D., Baltimore.
Francis Barton Gummere, Ph.D., Haverford, Pa.
Arnold Hague, Washington, D. C.
George William Hill, LL.D., Nyack, N. Y.
William Henry Howell, Ph.D., Baltimore.
Edward W. Morley, Ph.D., Cleveland.
Harmon N. Morse, Ph.D., Baltimore.
Edward Rhoads, Haverford, Pa.
Alfred Stengel, M.D., Philadelphia.
William Trelease, Sc.D., St. Louis.

Foreign residents—
Anton Dohrn, Naples.
Edwin Ray Lankester, LL.D., F.R.S., London.
Sir Henry E. Roscoe, F.R.S., D.C.L., London.
Joseph John Thomson, D.Sc., F.R.S., Cambridge, Eng.
Hugo de Vries, Amsterdam.

AFTERNOON SESSION, 2 P.M.

Vice-President SCOTT in the Chair.

The following papers were read:

"Further Notes on the Santa Cruz Edentates," by Prof. William B. Scott, of Princeton.

"Some Magnetic Properties of Nickel," by Mr. Joseph Wharton, of Philadelphia.

"A New Fresh-Water Molluscan Faunule from the Cretaceous of Montana," by Mr. T. W. Stanton, of Washington. Introduced by Prof. W. B. Scott.

"The Earliest Differentiation of the Egg," by Prof. Edwin G. Conklin, of Philadelphia.

"The Evolution and Distribution of the Proboscidea," by Prof. Henry F. Osborn, of New York.

"An Attempt to Correlate the Marine with the Non-marine

Jurassic and Cretaceous Formations of the Middle West," by Prof. John B. Hatcher, of Pittsburg.

"Hints on the Classification of the Arthropoda, the Group a Polyphyletic One," by Prof. Alpheus S. Packard, of Providence.

"Anatomy of the Floscullariidæ," by Prof. Thomas H. Montgomery, Jr., of Philadelphia.

"A Résumé of the Composition of Petroleum from Different Fields," by Prof. Charles F. Mabery, of Cleveland.

APRIL 4.—MORNING SESSION, 10 A.M.

President SMITH in the Chair.

The following papers were read:

"The Most Insidious Source of Error in Quantitative Chemical Research," by Prof. Theodore W. Richards, of Cambridge, Mass.

"A Further Classification of Economies," by Prof. Lindley Miller Keasbey, of Bryn Mawr, Pa.

"Some Features of the Supernatural as Represented in Elizabethan and Jacobean Plays," by Prof. Felix E. Schelling, of Philadelphia.

"The Hamites and Semites in the Tenth Chapter of Genesis," by Prof. Morris Jastrow, Jr., of Philadelphia.

"The Warfare Against Tuberculosis," by Dr. Mazÿck P. Ravenel, of Philadelphia.

ON A NEW GENUS OF HYDROID JELLY-FISHES.

BY WILLIAM KEITH BROOKS.

(Plate I.)

(*Read April 4, 1902.*)

GENUS DICHOTOMIA.

Diagnosis of the Genus.—Hydroid jelly-fishes with four radial canals which divide dichotomously two, three, four, or more times,

and open into the circular canal by sixteen, thirty-two, or more distal branches; with two sorts of tentacles—hollow ones and solid ones; with a simple mouth and with a single circumferential gonad which extends from the wall of the manubrium on to the radial canals and their branches.

Dichotomia cannoides (Plate I, Figs. 1, 2 and 3).

Diagnosis of the Species.—Bell subcylindrical, somewhat higher than wide, with a conical apex. Manubrium fusiform, widest at about the middle of its upper half. The four radial canals branch dichotomously four (or more?) times. Near the apex the four primary canals arise in two pairs from the ends of a short transverse canal. There are sixteen long hollow tentacles, and about thirty-two (or more?) short solid tentacles. The reproductive organ extends from the wall of the manubrium on to the radial canals and their branches for about half their length.

Special Description.—The four radial canals do not arise independently and directly from the aboral end of the stomach, but in pairs from the ends of a short transverse canal, in such a way that the only planes which divide the jelly-fish into symmetrical halves are the two primary interradial planes. When it is divided in either of these planes each half is itself bilaterally symmetrical, consisting of halves which are reversed copies of each other. In all my larger specimens each of the primary radial canals was divided dichotomously three times, so that there were eight secondary canals, sixteen tertiary and thirty-two terminal branches. In one specimen, which is shown in Fig. 1, one of these terminal canals was again divided into two, so that there were thirty-three instead of thirty-two terminal branches. It is therefore probable that the number of branches continues to increase with the age of the jelly-fish, and that older specimens may have sixty-four or more terminal branches. The subumbrella consists of two strongly contrasted regions: an upper opaque portion which is nearly hemispherical and which contains the arches formed by the reproductive organ on the arched subdivisions of the radial canals, and a lower portion which is cylindrical and transparent. About one-half of the total length of the system of canals is joined to the reproductive organ, which extends from the wall of the manubrium to the radial canals in a system of groined arches, dividing the upper part of the subumbrella into pockets which are closed above, open

below, and equal in number to the terminal branches of the radial canals. All of these pockets open at the same level below, but the sixteen pockets of the fourth set are very shallow, the eight pockets of the third set and the four of the second set are deeper, and the four primary pockets of the first set reach nearly to the apex of the subumbrella. The primary tentacles are stout, hollow, contractile, and when the jelly-fish is swimming they are stiffly extended with their tips coiled into compact spiral whorls. There are sixteen of these tentacles in every specimen that I have examined. The young specimen which is shown in Fig. 2 has sixteen distal radial canals, and a hollow tentacle arises from the circular canal in the plane of each branch of each radial canal. In the older specimen which is shown in Fig. 1 the hollow tentacles are still sixteen in number, although the distal canals are twice as numerous and although the hollow tentacles are now in the radii of dichotomy instead of being, as they are in the younger specimen, in the radii of the distal branches. The solid tentacles are short with little power of extension or contraction; they are usually turned outward and upward over the margin of the bell, and they remind one of the solid tentacles of the Geryonidæ. In all the specimens that I have examined they are equal in number to the distal branches of the radial canals: sixteen in the young jelly-fish shown in Fig. 2, thirty-two in those with thirty-two canals and thirty-three in the one shown in Fig. 1.

Color.—The gonads and the manubrium of old specimens are opaque white. The bell and the subumbrella and the tentacles are nearly colorless. The radial canals, the circular canal and the axes of the hollow tentacles are colored in young specimens by pigment-granules of a brownish-orange.

Size.—The bell is about one-third of an inch high and a little less than one-fourth of an inch in diameter.

Locality.—Several specimens were taken at high tide in an inlet from the open ocean in the Bahama Islands, near Nassau, in 1887, and at Bimini and at Green Turtle, in the Bahama Islands, in 1886 and 1888. It is common and widely distributed among the Bahama Islands.

If the analytical key which Haeckel gives in his *System der Medusen* were to be followed, the genus Dichotomia would belong among the "Leptomedusæ," in the family Cannotidæ, in the subfamily Williadæ, and in or near the genus Proboscidactyla (*System*

der Medusen, p. 158), although it is so different from the Williadæ and in fact from all the "Leptomedusæ" that it may turn out to be a tubularian jelly-fish, or "Anthomedusa." The simple manubrium and mouth, the hollow tentacles and the origin of the gonad in the wall of the manubrium are all points of agreement with the "Anthomedusæ." The solid tentacles have an axis made up of a single row of chorda cells, but as tentacles of this sort are found in undoubted tubularian jelly-fishes they afford no ground for excluding the genus Dichotomia from this group.

Prof. Walcott has described, from the Lower Cambrian of Alabama, certain remarkable fossils (*Fossil Medusæ*, by Charles Doolittle Walcott: Monographs of the United States Geological Survey, xxx, Washington, 1898) which he regards as the remains of Medusæ, and it is worthy of note that if the Medusa which is here described were slightly distorted by pressure the digestive and reproductive organs would exhibit some resemblance to one of the surfaces of some of Walcott's most characteristic types. At his suggestion I made a model in clay of the reproductive organs of Dichotomia in order to exhibit this resemblance, and Fig. 3 was drawn from this model. The resemblance lends additional support to the opinion that the Cambrian fossils are the remains of Medusæ, although it does not indicate that there is any relationship between Dichotomia and the fossils. In fact the resemblance is only superficial. In all the general details of their structure the Cambrian Medusæ must have been very different from the one that is here described.

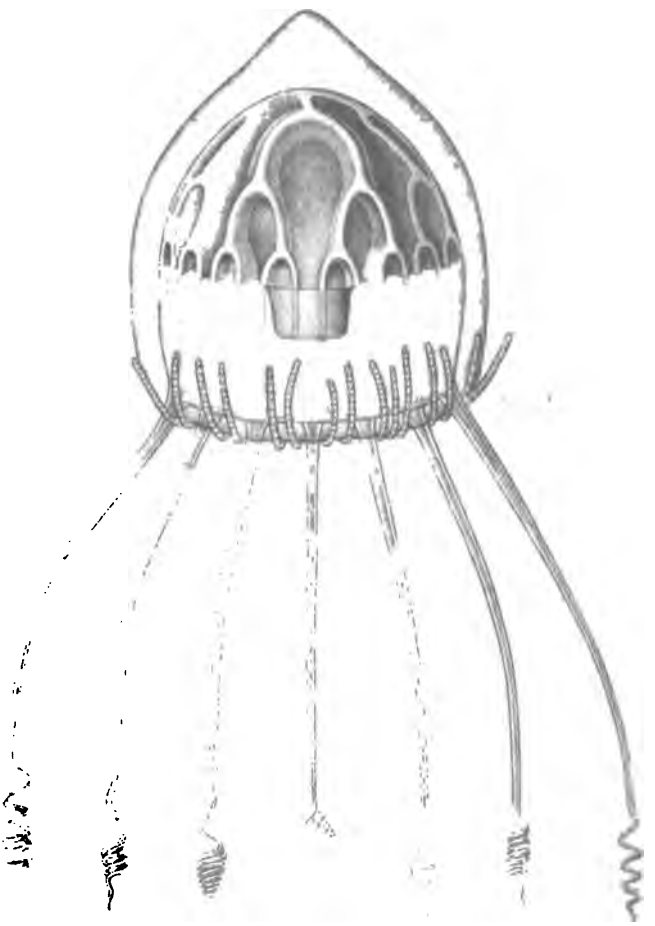
The notes and drawings for this paper were made in 1888, although I have been forced to delay their publication.

Explanation of Plate I.

Fig. 1. An adult specimen of *Dichotomia cannoides*, enlarged about six diameters. From a drawing made at Nassau, New Providence, in 1886.

Fig. 2. A young specimen, enlarged about twenty diameters. From a drawing made by R. P. Bigelow at Bimini, Bahama Islands, in 1887.

Fig. 3. A clay model of the reproductive organs of Fig. 1.







DICHOTOMA ANNULATA.

TO FACE PAGE 14

THE PROBLEM OF THE TRUSTS.

BY C. STUART PATTERSON.

(*Read April 2, 1903.*)

The "Trusts" in the popular, though not in the technical acceptation of the term, are the trading corporations, and combinations of such corporations, which control the production or the sale of one or more natural or manufactured products. "Trusts" thus defined do not include banks, nor other merely financial institutions, nor those "Public Service Corporations" which exercise the power of eminent domain, or which occupy public highways for their own purposes, as by laying rails, pipes or conduits.

The "Trusts" are the necessary result of an industrial evolution, whose successive stages have been individual ownership, partnerships, limited partnerships, corporations and combinations of corporations. Partnerships supersede individual ownership because of the advantages of the combination of the capital or services of two or more individuals. Limited partnerships are organized to facilitate the borrowing of capital by giving to the lender a compensation for the use of his money exceeding the current market rate of interest, while limiting his liability to creditors to the amount of the capital loaned. The same advantages of co-operation with limitation of liability, and with added exemption from adverse and discriminating regulation in other States, can be secured by association under "the Boston Trust Device."[1] Corporations are organized to assure continuity of administration; to obtain by the issue of shares, and sometimes bonds, the use of capital contributed by a larger number of persons than can conveniently be brought together as partners; to secure to shareholders exemption from the individual liability *in solido* of partners; and to render the shares negotiable *inter vivos*, and transferable, after the death of the owner, to personal representatives, or legatees, without the intervention of a court of equity in the settlement of partnership accounts.

In recent years the country has enjoyed a degree of material prosperity which has far exceeded the most hopeful anticipations.

[1] *The Financial Chronicle*, Vol. 75, p. 314, article by Richard W. Hale, Esq.

the minimum of cost can only be accomplished by securing a constant supply of raw material, by making promptly every alteration and improvement in plant and machinery which can effect greater economy in operation, by offering inducements to managers of superior administrative ability, by giving steady employment to workingmen at just wages, by accumulating a reserve fund available for extraordinary expenditures, by increasing the output so as to decrease the cost of production per unit, by expanding trade by creating new avenues for it, and by reducing the price to the consumer, while increasing the profit to the producer. To attain these cost-saving and profit-producing results, combinations of corporations and of properties have been and are being effected, in some cases by the merger and consolidation of existing corporations, and in other cases by the organization of new corporations, to hold and acquire the properties to be consolidated, or to obtain a controlling interest in the shares of the corporations to be combined.

For this there is needed the command of more capital than can be contributed by any one individual or by any group of individuals. Clear-sighted men saw that the prosperity of the country had not only made great fortunes for a comparatively small number of individuals, but had also aggregated in the deposits in the saving funds, in the accumulated reserves of the life insurance companies, and in the deposits in the banks and trust companies a fund of enormous and steadily increasing size, which must necessarily seek profitable investment, and which could be relied upon to make a market for the bonds and shares of corporations with a reasonably probable earning capacity, either by direct investment in those securities or by loaning funds for investment therein; and the appeal was successfully made to these new reserves of loanable funds. Therefore, to-day the capital which is operating the railroads, mining the ore and running the mills of the country is not provided by the rich men of the country, but is the accumulation of the savings of labor.

The test of the investment value of an industrial security is its reasonable probability of a continued earning capacity adequate to the payment of fixed charges and dividends at a rate exceeding that yielded by securities of a higher grade; and that probability of continued earning capacity will be affected by the relation between the cost and the real value of the properties bought for combination by the moderation or extravagance of the compensation given to

the promoters and underwriters, by the soundness of the principles upon which the combination is organized and conducted, and by the freedom of the business from governmental interference. It is not surprising that the number of the industrial organizations and the magnitude of their operations should arouse the public interest and should cause more or less fear as to possible consequences. Every great industrial development has excited such fears. The steam engine, the railways and all forms of labor-saving appliances, from the spinning jenny to the typesetting machine, have seemed n their turn to threaten large additions to the ranks of the unem ployed and heavy losses to different classes of people; and yet, in each case, the result has been the opening of new avenues to employment and a substantial advance in civilization. So to-day no one who is accurately informed as to present industrial conditions can doubt that, because of American financial skill in securing combination of resources and concert of action, the products of industry have been brought to a higher standard, the labor which produces them is better paid than ever before, and the consumer buys them upon relatively more favorable terms.

Concurrently with the organization of corporations with increased capital for production, manufacture and trade, there have come into operation unions of laborers of larger membership and greater activity. Every one ought to concede that it is right that workingmen should receive full and adequate compensation for their labor and should have that legally guarded freedom of contract which will enable them to sell their labor to the best advantage. Every one ought to concede also that workingmen should form associations for the protection of their interests and to secure increases in their wages; but it is not right that those unions should undertake to reduce the mass of workingmen to a low level of mediocrity by means of limitations of the hours of voluntary labor, and by restrictions upon the quantity and quality of work to be performed by the individual laborer; nor is it right that the unions should attempt to monopolize the supply of labor by preventing the employment of non-union laborers. Such limitations and restrictions aggrandize the labor leaders, but they degrade the workingmen; they tend to deprive intelligent, industrious and ambitious workingmen of the opportunity to rise out of the ranks; they diminish that effectiveness of American labor which has been not the least of the causes of the country's industrial supremacy; and

they endanger the continuance of our present prosperity. Combinations of laborers for such purposes are monopolies in restraint of trade.[1] The law should clearly recognize the right of association, but it should also require the incorporation and registration of labor unions in order that there may be legal responsibility for broken contracts; it should, in cases of strikes, impartially enforce law and maintain order, and it should sternly repress any attempt, by whomsoever made and howsoever made, to hinder men in the exercise of their right to work. We find nothing in party platforms, and we hear nothing from executive officers, legislators or candidates for office as to the need of legislative or executive action to curb the power of the labor leaders or to restrain the excesses of their misguided followers. But we hear much of the need of executive and legislative control and regulation of industrial corporations.

It is said, and it is doubtless true, that some industrial organizations are over-capitalized. In considering the capitalization of a corporation, it must be remembered that whatever be the par of a share of stock, that par is not, like the par of a bond, a principal obligation, to be discharged in a certain number of dollars, but it is only the right to an *aliquot* proportion of the net assets of the corporation, and it really represents an amount in dollars, more or less than its par, in proportion as those assets when liquidated shall realize more or less than the aggregate capital of the corporation. The market price expresses the investors' valuation of that representation as affected by the demand for and supply of the particular stock.

The authorized capitalization of a corporation is, in general, determined by the probable cost of the plant, with the addition of the amount of working capital in money necessary for the successful conduct of the business to be transacted, and sometimes with the further addition of a capitalization of the estimated earning capacity of the plant. Obviously, the issued capital should not, in a properly managed corporation, exceed at any time the actual value of the plant and working capital as determined by present and not prospective earning capacity. In other words, there should be no watered stock.

[1] *In re* Debs, 64 Fed. Rep., 724, 745, 755; 158 U. S., 564; *The Law of Contracts in Restraint of Trade, with Special Reference to Trusts*, by George Stuart Patterson, Esq.

It is asserted by no less eminent an authority than the distinguished lawyer who is now the Attorney General of the United States[1] that the necessary effect of "over-capitalization" is to unduly lower the wages of labor and to unduly increase the prices of the product by imposing upon the managers of the corporation the obligation of paying dividends upon the improper excess of capitalization. This view does not seem to be reasonable, for it would obviously be the effort of the managers at all times to keep down the expenses of operation, to make as many sales as possible, and to realize as large prices as possible, without reference to the interest or dividends to be earned or paid, if for no other reason than that of demonstrating the ability of the managers, and this effort would neither increase nor diminish because of the greater or less size of the capitalization. Indeed the only possible influence of a large capitalization upon the prices of the product of the over-capitalized corporation would be in some cases to cause a lowering of such prices, because of the necessity of making realizing sales.

Complaint is made that competing corporations, in order to destroy competition, discriminate in their prices. But competition is industrial warfare. The seller seeks the highest price that he can obtain; the buyer pays as little as he possibly can. When competition is actively conducted, the seller attains his ends, not only by underselling in order to effect a particular sale, but also by carrying his underselling to the extreme limit of driving his competitors out of business and securing for himself complete control of the market. This is done, as Lord Justice Bowen said,[2] from "the instinct of self-advancement and self-protection, which is the very incentive of all trade." . . . . "To say that a man is to trade freely, but that he is to stop short at any act which is designed to attract business to his own shop, would be a strange and impossible counsel of perfection," and to attempt to prohibit it "would probably be as hopeless an endeavor as the experiment of King Canute." You cannot have a real competition which does not compete. Successful commerce buys in the cheapest and sells in the dearest market. Is it proposed that there shall be a general legislative regulation of prices, and if so, what would that amount to?

Among the evils charged against the "Trusts" are an alleged

[1] Speech of Hon. P. C. Knox, Pittsburg, 14 Oct., 1902.

[2] Mogul S. S. Co. *v.* McGregor, 23 Q. B. Div. 598; [1892], C. A. 43.

"insufficient personal responsibility of officers and directors for corporate management." It is enough to say as to this that the laws of some States do, and the laws of all States can, hold corporate managers to a strict responsibility for non-feasance and mal-feasance.

It is also said that these trade organizations have "a tendency to monopoly," but except in the cases of patents and copyrights, and of those who control the sole and exclusive source of supply of a natural product, it is not possible in this day of the world's history to maintain and enforce more than temporarily extortionate prices, for the reason that there is always available a large amount of uninvested capital seeking profitable employment and keenly watching for opportunities of remunerative investment, and therefore intelligent managers of a successful business do not advance prices to a point at which destructive competition will be invited. Prices of commodities are automatically regulated by the law of supply and demand. When, by reason of an apparent permanence of demand and a present inadequacy of the means of supply, prices rise to a level that gives a reasonable assurance of profit to producers, the surplus capital of the world can always be relied upon to augment the means of supply.

It would seem that there is a popular demand for industrial organizations sufficiently strong to overcome repressive legislation. Beginning with the Congressional Anti-Trust law of 2d July, 1890,[1] and the Mississippi statute of the same year,[2] and ending at this time with the sixth section of the United States statute creating the Department of Commerce and Labor, the United States and twenty-nine States and Territories have legislated upon the subject, and two States have deemed it of sufficient importance to place an anti-Trust prohibition in their Constitutions. Some of these State statutes regulate and others of them prohibit, and all of them impose penalties by fine or imprisonment and forfeiture of corporate privileges. Notwithstanding this mass of adverse legislation, industrial corporations have grown and flourished; and yet there is a political clamor for more legislation.

Attempts to regulate trade by legislation are not of new invention. Whenever and wherever there has been an absolute government there have always been attempted restrictions upon trade. In

[1] 26 Stat. 209, c. 467.
[2] c. 36.

mediæval times it was the theory and the practice that it was the "duty and the right of the State to fix hours of labor, rates of wages, prices, times and places of sale and quantities to be sold."[1] The selfish commercial policy of England, intelligently directed to the restraint of colonial trade and manufactures, was the great cause of the War of Independence. When the success of the War of the Revolution had substituted the sovereignty of the people for the supremacy of the Crown, there was naturally a jealousy of governmental power and a determination to guard individual liberty against oppression. The framers of the Constitution of the United States therefore founded the government, not only upon the supremacy of the federal government in the exercise of the powers granted to it, but also and equally upon the independence of the States and the freedom of the citizen. They foresaw the evil effects of an unrestrained exercise of the popular will. They endeavored to establish and make perpetual the reign of law. They crystallized into the Constitution the great principles of free government, and they made it impossible to hastily change that organic law. They declared in express terms the supremacy of the Constitution and the laws made in pursuance thereof; and they created a Supreme Court whose judgments should give effect to that declaration. They united the States in a nation, with full powers of government for all national purposes, but they retained the sovereignty and independence of the States for all purposes of local self-government, and they reserved to the individual citizen as much freedom as is consistent with the enforcement of law and the maintenance of order. While they granted to the United States the power of regulating commerce, they limited the exercise of that power to foreign and interstate commerce.

It is to the States, and not to the United States, that we ought to look for the legislative and administrative regulation of the industrial organizations of the present and the future. The power of the State is ample. A State may create corporations, with or without conditions, and it may authorize a corporation to do any business which an individual may lawfully do. A State may forbid a foreign corporation to do business within its territory; it may permit that business on conditions; and it may, with or without reason, revoke a permission theretofore granted.[2] It may, therefore,

[1] Mrs. Green, *Town Life in the 15th Century*.

[2] W. P. O. Co. *v.* Texas, 177 U. S., 28.

enforce with regard to foreign corporations all, and more than all, the restrictions which it enforces with regard to corporations of its own creation. On the other hand, the United States, save as the domestic government of the District of Columbia and the Territories, cannot even grant a charter of incorporation, except as a means incidental to the exercise by the United States of a power of government,[1] and it can, but only under the power of regulating foreign and interstate commerce, control the operations of a corporation chartered by a State. It does not avail to say that the legislation of a State can have no extra-territorial force, and that in order to have a rule of uniform application throughout the country there must be Congressional legislation, for the conclusive reply is that every State, under the Constitution, is entitled as of right to determine for itself by what agencies and under what conditions commodities shall be manufactured or sold within its territory,[2] subject only to the paramount right of the United States to levy duties and taxes, and to regulate transportation. Nor is it to the interest of the people that a Constitutional amendment should be adopted vesting in the United States the proposed power of regulation. The fatal facility of compromise as shown in the history of the slavery question and the silver issue, and exhibited in every tariff act, and the lack of appreciation of the demands of legitimate business as evidenced by the failure of Congress to do away with the antiquated sub-treasury system and to authorize an elastic currency, demonstrate the unwisdom of vesting in Congress a power which, if exercised, may injuriously affect the business interests of the country.

So far as concerns Congressional legislation under the Constitution as it now is, it would seem that the Supreme Court has put the question at rest, for it has decided[3] that "the relief of the citizens of each State from the burden of monopoly and the evils resulting from the restraint of trade among such citizens were left with the States to deal with;" and that an organization to manufacture and sell is a subject matter of State regulation, for the reason that, while it may bring the operations of commerce into play, it affects

[1] McCulloch *v.* Maryland, 4 Wheat., 316.

[2] U. S. *v.* De Witt, 9 Wall., 41; McGuire *v.* The Commonwealth, 3 *id.*, 387; Patterson *v.* Kentucky, 97 U. S., 501.

[3] U. S. *v.* E. C. Knight Company, 156 U. S., 1, 11.

commerce only incidentally and indirectly. That judgment is unreversed and unshaken, and it is to-day the law of the land.

In the past, the country has had to overcome, under conditions of inadequate transportation facilities, the disintegrating tendencies of the expansion of territory and the growth of population, but as the results of the triumph of the nation in the suppression of the Rebellion, and the development of means of transportation and communication, our perils are now those of governmental consolidation and not those of dissolution. The silver legislation, threatening a degradation of the standard of value; the income taxing law of 1894, unnecessarily vexatious and inquisitorial in its provisions and, by reason of its exemption of incomes less than $4,000 in amount, unjust and unequal in its intended operation; and the laws imposing taxes upon inheritances, increasing progressively in proportion to the amount of the distributive share and teaching the many to expect that the necessary expenditures of government will be borne by taxation to be levied on the few, are illustrations of the perils which may threaten the prosperity of the country. Any legislation which conflicts with the American doctrine that all men are equal before the law, and that equality of rights implies equality of obligations, and that subjects rights of property and freedom of contract to administrative control, is dangerous in a republic governed by universal suffrage.

Every State should encourage the organization under proper conditions of manufacturing and trading corporations, and it should permit them to do any business that an individual may lawfully do. While a State should not undertake to guarantee to the public "in all particulars of responsibility and management" the corporations organized under its authority, it should by conditions annexed to the grant of the charter protect intending and actual shareholders and creditors of the corporation, so far as can be done. It should prohibit and punish fraud in organization or administration. It should afford access to public records showing how the capital has been or is to be paid, and if paid in property, so describing and identifying that property that its value can be tested. It should require the publication at stated periods of balance sheets stating the liabilities under the headings of "shares," "bonds," and "other indebtedness," and the assets under the headings of "real estate and machinery at cost" less stated amounts charged off for depreciation, "cash," "debts collect-

ible," and other "assets," and it should compel the publication at stated periods of an income account, setting forth the total amounts of gross earnings, of operating expenses, interest charges, dividends, and undivided profits. It should also protect minority shareholders by defining the powers of officers and directors, and by requiring due notification of shareholders' meetings.[1] The duties imposed upon the corporations and their officers and the requirements as to reports should be fixed by law, and should not be subjected to the discretion of public officers. If so subjected, there will be a possibility of favoritism on the one hand, or of political intimidation on the other.

The officers and directors of a corporation are trustees for its creditors and shareholders. It is the duty of such trustees, and the interest of their *cestui que trusts*, that there should be, on the part of the trustees, absolute good faith, and as much frankness as is consistent with the protection of the rights of shareholders. No other policy will commend the securities of a corporation to the favorable consideration of investors. Nevertheless, it is certain that no amount of legally enforced publicity will avail to prevent unwise investments and consequent loss. Those who may properly encounter the risks of investment in industrial securities will not do so before they shall have obtained for themselves information as to honesty of organization and adequacy of earning capacity. Those in whom the haste to become rich, and the disinclination to wait for the slow but sure results of industry and frugality have caused a fever of speculation, will take the chances and buy for a rise in the market without regard to the condition of the corporation, real or reported.

Neither the government, nor the public, nor even the purchasers of Trust products provide the capital of the Trusts, but that capital is contributed by their bondholders and shareholders. In so doing, they are not making investments which are of certain return, either in principal or income. As they take the risk of loss, they are entitled to security in their business, to protection in the use of their properties, and to a return proportioned to the risk they have taken and exceeding the current market rate of interest upon securities of a higher grade.

[1] 1903, Report of the Massachusetts Committee on Corporation Laws, by H. M. Knowlton, C. G. Washburn, and Frederic J. Stimson.

While the corporation is a legal entity, distinct from the bondholders and shareholders who contribute its capital,[1] nevertheless the fact remains that the bondholders and shareholders are the persons, or the successors of the persons, whose contributions in money or in property, real or personal, constitute the capital of the corporation, and it is they, or their successors in title, who are benefited by the prosperity of the corporation or injured by its adversity. In the days of the "greenback" agitation, the "bloated bondholder" was the object of fierce denunciation, but when the facts were investigated it was found that the "bloated bondholders" were the saving funds, with whom were deposited the accumulated earnings of labor, the insurance companies, to whom every provident man looked for the future protection of his dependent wife and children from want in case of his death, and the banks, with whom all business men, great and small, deposited the money which they needed for daily personal and business use. Again, when the banks were assailed as heartless money lenders and oppressors, it was seen that the profits of banking accrued to the men, women, and children, who were the shareholders. Again, when Mr. Bryan, in 1896 and in 1900, denounced the holders of "fixed investments" and "the idle owners of idle capital," intelligent people knew that those whose interests he threatened with destruction were the depositors in saving funds, the shareholders in banks, and the holders of policies of life insurance. It will likewise be found to-day that those whose interests will be injured by unwise legislative restraints upon trade and industry will be the workingmen to whom the Trusts offer employment, and the hundreds of thousands of individuals who are, directly or indirectly, the owners of the securities of the Trusts. The leaders of public opinion will do well to remember that, as Mr. Lecky has said,[2] it is an inexorable condition that all "legislation which seriously diminishes profits, increases risks or even unduly multiplies humiliating restrictions, will drive capital away and ultimately contract the field of employment."

[1] Regina *v.* Arnoud, 9 Q. B. 806, 58 E. C. L.; Van Allen *v.* The Assessors, 3 Wall., 573.

[2] *Democracy and Liberty*, Vol. II, page 463.

THE INCLUSION AND OCCLUSION OF SOLVENT IN CRYSTALS.

An Insidious Source of Error in Quantitative Chemical Investigation.

BY THEODORE WILLIAM RICHARDS.

(*Read April 4, 1903.*)

Perhaps the most frequent and the most insidious cause of error in quantitative chemical research is the unsuspected presence of water. The disturbing effect of this impurity is frequent, because water is one of the most plentiful of substances; and insidious, because there is usually no easy quantitative or qualitative test for small quantities of it.

The object of this paper is to recount several experiments indicating the prevalence and magnitude of inaccuracy from this cause, to show how many published results have been vitiated by it, to emphasize the theoretical aspect of the phenomenon, and especially to point out the methods of reducing the inaccuracy to a minimum.

In a number of isolated cases it has been shown by various chemists that substances crystallizing from a solution enclose within their crystals small quantities of the mother-liquor. It is very well known, for example, that crystals of common salt explode or decrepitate upon being heated, because of the vaporization of the enclosed water. Thus when common salt is weighed some water is weighed with it. This water is not combined as water of crystallization, of which common salt has none; it is entirely accidental. Following the nomenclature of the mineralogists, a liquid imprisoned in this way is best called "Included solvent." The mother-liquor is thus imprisoned, in addition to combined water, also in salts which contain this latter as an essential part of their crystal structure; in these cases it is even more difficult to detect and more generally overlooked, because of the simultaneous presence of the combined water.

Although these facts have been thus occasionally pointed out, the frequency of the occurrence of this cause of error has not often been realized by quantitative analysts. It is no careless exaggeration to state that in all my chemical experience I have never yet obtained crystals from any kind of solution entirely free from accidentally included mother-liquor; and, moreover, I have never found reason to believe that anyone else ever has. Whether the

solvent is water, an organic liquid, or a fused salt at high temperature, and whether the crystallization is quick or slow, in small or large crystals, the effect is always traceable, although of course in varying degrees. The amount of the enclosure varies from perhaps 0.01 per cent. to 0.5 per cent. of the total weight of the crystals.[1]

As a general rule, the clearer the crystal, the less the included mother-liquor; but appearance is not a wholly safe guide, since sometimes the refractive index of the mother-liquor is not far from that of the crystal. In this case the included impurity is invisible. Moreover, the inclusion may, and undoubtedly does, often occur in cells so small as to be beyond the reach of the best microscope.

It is not contended that the production of a perfectly pure crystal from a solution is impossible, but only that no evidence has been obtained proving that this end has ever artificially been reached, while much contrary evidence is available.

In looking over the records of the determinations of atomic weights, it is surprising to see how many experimenters in even this most exact field of quantitative analysis have either entirely overlooked the danger, or have taken inadequate means to overcome it. This is especially true when the salts to be weighed contain combined water of crystallization; indeed it is almost safe to rule out utterly all such results, without further consideration. Among other more or less vitiated cases, where salts supposedly anhydrous have been weighed, may be mentioned especially a number of the analyses of typical salts of osmium, iridium, platinum, and palladium. Often a painstaking but unthinking chemist has spent months in eliminating the *hundredth* of a per cent. of some foreign metal and finally ignored the *tenth* of a per cent. of water in his preparation. It is not, however, the purpose of this paper so much to seek the errors in individual instances of past work, as to point out the ways in which these errors may be avoided in the future.

How then is this included solvent to be eliminated without decomposing the substance which we desire to weigh?

It is usually considered as a sufficient precaution to powder the material finely and expose it to the air for a short time, in order to allow the undesirable water to evaporate. This crude proceeding involves a double uncertainty; in the first place, the unwar-

[1] For examples of carefully obtained evidence, see Richards, *Proc. Amer. Acad. Arts and Sciences*, 23, p. 177 (1887); 26, p. 267 (1891); 28, p. 11 (1893); 29, p. 60 (1893); 33, p. 299 (1898); 35, p. 139 (1899); 37, p. 434 (1902); 38, p. 411 (1902).

ranted assumption is made that every hidden cell containing the mother-liquor has been split open by the pestle, and in the next place the equally unwarranted assumption is made that all the mother-liquor thus exposed evaporates into the uncertain mixture constituting the atmosphere of the laboratory.

The former of these assumptions will be considered first. Is it possible to open all the cells enclosing mother-liquor by means of any finite amount of powdering?

This question cannot be answered *a priori;* accurate experiments are needed to decide it, and no published work known to me seems to furnish the needed data. Accordingly a series of experiments was planned which involved the progressive powdering of a typical substance. In order to separate the powders of different degrees of fineness four pieces of brass netting were used, having openings about 0.5, 0.3, 0.23, and 0.16 mm. in diameter respectively. This netting was cleaned with acid and ammonia, and showed a brilliantly clean surface in the microscope.[1]

The test substance chosen was baric chloride, because careful experiments on the atomic weight of barium had shown that it may be analyzed with ease and accuracy. A pure finely-crystallized specimen of the salt was slowly and carefully powdered and sifted through the successive sieves. That which went through the finest sieve was still more finely powdered, until the average diameter of the particles as estimated in the microscope was perhaps one-twentieth of a millimeter, some being coarser and some finer. Each specimen was thoroughly air-dried.

Thus were obtained four examples of baric chloride containing crystal-water, of four different degrees of fineness, the coarsest particles averaging about a thousand times the bulk of the finest. Upon analysis by heating to constant weight at 400° these samples yielded respectively 14.780, 14.771, 14.763, and 14.760 per cent. of water.

The data, reduced to the vacuum standard, were as follows. The experiments were made in 1893:

No.	*Average diameter of particles.*	*Weight of salt.*	*Loss on heating.*	*Per cent. of water found.*
1	0.45 mm.	4.15929	0.61475	14.780
2	0.27 mm.	3.65127	0.53933	14.771
3	0.20 mm.	4.54136	0.67047	14.763
4	0.05 mm.	10.13720	1.49620	14.760

[1] The material sifted through this netting was tested for copper, with satisfactorily negative results.

The results thus show a steady decrease in the amount of water as the powder becomes finer; hence each successive powdering must have opened new cells.

When the figures are plotted a somewhat irregular curve is obtained, the study of which seems to show that further powdering would have but little effect upon the last-named amount of water held by the crystals. As a matter of fact, among many scores of determinations of the quantity I have never found in a pure specimen of baric chloride less water than this. Is this limit then, observed in the very fine state of division, the true amount of water in the salt? This question is easily answered in the negative, for from the universally accepted atomic weights of barium, chlorine, oxygen and hydrogen it is easy to calculate that the theoretical amount of water is 14.744 per cent., an amount less by one part in a thousand than the lowest limit recorded above.

Similar results were obtained from cupric sulphate, and it seems probable that any other salt would behave in the same way. Thus it appears that although powdering and drying will diminish the excess of solvent, it will not wholly remedy the error. It is probable that anything short of molecular division would fail to open the minutest enclosing cells, although the larger cells are broken up with comparative ease.

The irregular shape of the curve drawn from the data given suggests that there is another cause of error superposed upon the effect just studied—an influence which grows in magnitude as the fineness of the powder is increased. A moment's reflection serves to show what this new cause of error must be.

It is well known that water adheres to or wets almost anything, except a few fats and oils; and even these absorb or dissolve water to some extent. In consequence of this tendency to adhere, water is condensed or *adsorbed* in an invisible film, from the always slightly moist atmosphere, upon the exposed surface of nearly all substances. This adhering film increases the weight of these substances.

The extent of the adsorption varies with the nature of the substance and in many cases is easily appreciable. With a given substance it is of course dependent upon the pressure of the aqueous vapor and the temperature, as well as upon the extent of surface.

Since the adsorption increases with the surface it is evident that fine pulverization, while tending to diminish the inclusion, will

tend to increase this new cause of error by increasing the field of its action.

Thus the irregularity of curve shown by the results with baric chloride is easily explained, as well as the abiding presence of an excess of water. In the effort to escape the Scylla of inclusion the chemist has run foul of the Charybdis of adsorption.

Adsorption of aqueous vapor can be eliminated by greatly raising the temperature or by greatly reducing the tension of the surrounding aqueous vapor. These means may be used either with anhydrous salts or with the sundry chemical vessels used for containing substances while weighing; but unfortunately either of these changes drives out also the crystal-water contained in a hydrated salt.

For these reasons it seems to me impossible to determine with the exactness demanded in the most accurate work the true weight of any salt containing water of crystallization.

In the case of *anhydrous* salts, which may be heated and placed in a perfectly dry atmosphere without decomposition, it is easy to eliminate adsorbed moisture, as already stated. It is not by any means so easy, however, to drive off the included solvent imprisoned in hidden cells. Some means must be used which disintegrates the walls of these cells; and the means adopted depends upon the nature of the substance being studied.

Either mechanical, thermal or chemical influences may be used to effect the disintegration. The mechanical means, pulverization, need not be further discussed. The application of heat first tends to vaporize the imprisoned solvent, if it is volatile, causing great pressure in the small space. This pressure often causes the enclosing cell to explode, and thus the solvent is set free. It is not by any means certain, however, that all the cells are thus able to discharge their contents, especially in tenacious substances; and in many cases hours of intense heating are necessary to drive off every trace of water, as with silica and iron rust. In the case of metals precipitated as such from aqueous solutions, temperatures not far from the melting-points must be used for drying, in order that their condition may be soft enough to yield to the internal pressure of the enclosed solvent. This precaution has usually been overlooked by physicists determining electro-chemical equivalents, although Lord Rayleigh pointed it out in the special case of silver twenty years ago.

A much safer plan, where it is practicable, is to *fuse* the cystalline precipitate. With the freedom of motion given by the liquid state volatile impurities usually soon escape, leaving the fused mass free from them. It is true that there is often danger that a portion of the substance itself will evaporate, or at least that it will attack the containing vessel; minute precautions must be used to avoid these causes of error. In extreme exigency the electrolysis of the fused mass, a plan suggested by Richard Lorenz, may be used as a means of destroying the last traces of water; but in most cases these may be swept out by the vapors from easily decomposed ammonium salts, just as bubbling air sweeps out carbonic acid from its solution.

A yet further and better thermal means of preparing a substance free from water is to vaporize and condense or sublime it in a perfectly dry atmosphere. Here crystals form under conditions excluding water, and the danger is wholly overcome. Unfortunately other impurities are usually introduced from the walls of the vessel used for the sublimation; but frequently these may be found by analysis much more directly and precisely than the water could be.

A chemical method of disintegrating the structure of a substance crystallized from a solution has been alluded to above. This method, although not always available, may sometimes serve when the other methods are inapplicable, and is often of great use in preparing chemically pure substances. This procedure, like the others, has been used in individual cases for years; but it does not seem to have been emphasized as a general method.

In brief the chemical method is as follows: The substance is crystallized from the solution, not directly in its desired form, but rather in chemical combination with a large quantity of some other substance which may be volatilized by suitable subsequent treatment. The clear, dry crystals are then subjected to this decomposing treatment, and the volatile constituent is expelled. The substance sought is thus left in the form of a porous mass, a skeleton of the former crystal, in which every cell enclosing mother-liquor has been opened by the chemical disintegration of its walls. From such a skeleton soluble impurities may often be washed out by lixiviation, and volatile ones escape at once.

For example, it is easier thoroughly to dry sodic carbonate when this salt is crystallized with its maximum amount of crystal-water,

than it is when the salt is crystallized immediately in the form devoid of crystal-water. Again, it is far easier to obtain pure iron by the reduction of the oxide in hydrogen than it is by electrolytic precipitation direct from a solution.

Yet another chemical method might sometimes be helpful, although inherently of a somewhat restricted usefulness. In a careful study of the behavior of gases included in oxides,[1] it was found that in these compounds oxygen could work its way out from a cell in which it exists under pressure, while nitrogen could not. It is possible that this fact is typical of a general principle; that any substance may escape from pressure when this substance forms one of the easily dissociated components of the containing walls, by a process of alternate dissociation and recombination of the materials constituting these walls. Thus certain hydrated salts, heated to a temperature of perhaps 120° in superheated steam at atmospheric pressure, might in time part with their enclosed water without losing their water of crystallization. This inference will be further tested in the near future.

In this connection, one other point must be strongly emphasized, because of its important bearing both upon chemical purification and upon dynamic geology. The microscopic cells in a crystal contain not only the solvent, but also all the other substances in the solution. They are miniature samples of the solution, not perhaps in strict quantitative measure, because of varying adsorption, but at least qualitatively.

Hence, if a chemically pure substance is desired, great pains must be taken to keep away from the solution anything which cannot be expelled or extracted from the disintegrated crystal, after the enclosures have been opened. For example, pure iron must be obtained by the reduction of oxide or hydroxide obtained from the nitrate, not from the sulphate. If the latter salt were used, the iron would probably contain sulphur. This practice of continually avoiding impurities obviates also the possible danger from "solid solution," as van't Hoff pertinently terms the *homogeneous* distribution of foreign matter in a solid. Solid solution, or *occlusion*, may be said to be the limiting case of *inclusion*. In this limiting case the enclosing cells are so small as to contain only single molecules. The distinction between occlusion and inclusion in solids is theoretically interesting, corresponding perhaps to the difference

[1] Richards, *Am. Chem. Jour.*, 20, p. 701 (1898).

between true solution and colloidal solution in liquids; but for present practical purposes this difference need not be further emphasized.

All these considerations have been carefully heeded in the recent determinations of atomic weights made in this laboratory.

Since the fundamental properties of material have probably not changed since the archæan times of mineral growth, natural crystals must have been subject to the same effects as those grown to-day, and all except those formed by sublimation must contain traces of the solutions from which they once separated. In some cases, of course, the very slow formation might reduce the inclusion to a very small amount. Those minerals coming from aqueous solutions would be supposed to contain accidentally enclosed water (often erroneously confounded with the true water of constitution), while those separating under fused or metamorphic conditions would contain non-volatile impurities taken from their immediate surroundings. Of these impurities the non-volatile ones must certainly remain; and many of the volatile ones may also, if closely enough imprisoned. As a matter of fact, we are very familiar with the traces of impurity in natural crystals, even the clearest of diamonds leaving some ash on combustion.

While the selective effects of adsorption and solid solution and the results of subsequent pressure must complicate the interpretation of these facts, and forbid their immediate quantitative utilization, it seems to me nevertheless that these enclosed traces of impurity might be used more often than they are used in geological reasoning, in order to discover the media from which crystals have separated, and hence the mechanism of their formation. Thus a phenomenon very troublesome to the chemist might become very useful to the geophysicist.

The contents of this paper may be summarized briefly as follows:

(*a*) Experiments are recorded and quoted showing that many, if not all, crystals separated from solutions contain included mother-liquor.

(*b*) The experiments show also that before the mother-liquor can be eliminated by pulverization the adsorption of water from a moist atmosphere begins to augment the weight of the substance.

(*c*) It is pointed out that this adsorption cannot be wholly overcome in the case of hydrated salts without a loss of water of crystallization also. Hence hydrated salts cannot be accurately weighed according to any usual procedure.

(*d*) In the case of anhydrous salts the elimination of adsorption is easy, but in order to remove included solvent the cell walls enclosing it must be disintegrated.

(*e*) Mechanical, thermal and chemical methods of such disintegration are classified and applied to the preparation of pure materials.

(*f*) It is pointed out that other impurities besides the solvent will usually be enclosed in the cells, and that these other impurities must never be forgotten in subsequent processes of purification.

(*g*) Finally, it is suggested that these enclosed impurities might be used more frequently than they are as a clue to the manner of growth of natural minerals, and hence to the mechanism of geophysical processes.

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A RÉSUMÉ OF THE COMPOSITION AND OCCURRENCE OF PETROLEUM.

BY CHARLES F. MABERY.

(*Read April 3, 1903.*)

I have said and written so much about petroleum during the last fifteen years, it may seem that I have reached the limit of interest and about exhausted the subject. Twenty years ago when I first went to Cleveland I began the study of petroleum, and have since devoted a considerable portion of my time to the examination of the constituents of petroleum from many different fields. But instead of exhausting the subject it is evident that only a beginning has been made, and the foundation for what is probably the most difficult and intricate parts of this interesting field of research.

The series of hydrocarbons which form the portions of petroleum distilling below 350° *in vacuo*, corresponding to 475° atmospheric pressure, are now well understood, and the members of the various

[1] The subject matter of this paper is based on the results of work carried on in the chemical laboratory of Case School of Applied Science, with aid of grants by the American Academy of Arts and Sciences from the C. M. Warren fund for chemical research.

series have been identified with respect to their molecular weights. But concerning the structure of these hydrocarbons, except those of the series C_nH_{2n+2} and the lower methylenes nothing whatever is known. It is reasonable to assume that the members of the series C_nH_{2n+2}, or the so-called paraffine hydrocarbons, have the open-chain structure which characterizes the lower members of this series. In earlier literature on petroleum it was generally assumed that the ethylene hydrocarbons, series C_nH_{2n}, formed a considerable proportion of the constituents, and even after the discovery of the series C_nH_{2n}, the naphthenes, according to the earlier nomenclature of Markownikow, many writers still insisted on the presence of the ethylene hydrocarbons. It is now safe to assert that these bodies are present in any petroleum at most in very small amounts. We have found them apparently in Canadian petroleum, but in very small quantities.

The series C_nH_{2n}, which has been identified in petroleum from many sources, is now well known as the methylene series. In a paper published last year on the composition of Pennsylvania petroleum, I purposely abstained from naming the hydrocarbons with high boiling points of this series which we had separated and identified, for although it seemed probable that these bodies were methylenes, I preferred not to suggest names for the several members until more is known concerning their structure. The names suggested by Dr. Bogert in his summary of the results described in that paper for the *Journal* of the American Chemical Society, seems to refer those hydrocarbons to the ethylene series; but any nomenclature for these bodies must await sanction by proof of structure when some courageous investigator shall force his way into this difficult field.

Another feature of the petroleum problem is the form of the hydrocarbons which form the highest boiling portions—the so-called asphaltic hydrocarbons. The main body of these high boiling oils are no doubt composed of series poorer in hydrogen than the methylenes, the series C_nH_{2n-2}, C_nH_{2n-4}, etc. The hydrocarbons of these series already appear in the higher boiling portions of Pennsylvania, Ohio, Canadian, etc., petroleum, as we have shown in part, although much of this data has not yet been published.

It does not at present seem clear how this problem shall be attacked. By exclusion of air and depression of boiling points the petroleum hydrocarbons can be distilled indefinitely as high as

350°. Between 300° and 400° cracking begins, and it cannot be avoided by straight distillation. The heavy hydrocarbons seem to become so inert, by reason of their high molecular weights, they cannot retain their atomic composition at their boiling points; they simply fall to pieces through the influence of mass. In distillation from the crude oil evidently another influence comes into operation—the effects of the oxygen, nitrogen and sulphur constituents. Since fractional distillation is the only means at present known for the separation of the homologous members of these series, the problem of their isolation becomes a difficult one.

Nevertheless I regard this field as offering great attractions, provided, as I mentioned some time ago, suitable facilities are provided for carrying on the work. A grant of $5000 annually from the Carnegie University could be made to yield results commensurate with the expenditure, for there is no more promising field for research of such magnitude awaiting a vigorous hand. As for myself, I shall be content with what I have been able to accomplish with the aid of the C. M. Warren fund and the facilities of the Case School laboratory in defining the series and principal members in petroleum from different fields that has come under my observation.

In presenting a general summary of present knowledge concerning the composition of petroleum, it may be of interest to refer to what was known on this subject twenty years ago when I began the work. At that time the only petroleum on the market in America was obtained from the Pennsylvania fields and the territory in Canada. The composition of Russian petroleum was then under investigation by Markownikow. As a result of their elaborate investigation on American petroleum Pelouze and Cahours had assigned the formula C_nH_{2n+2} as representing the principal series of hydrocarbons. But the high specific gravity of their distillates could not have been given by hydrocarbons separated from Pennsylvania petroleum, since these bodies give much lower values. Since the source of their products was not mentioned, it must be assumed that they came from the heavier Canadian oil, although the hydrocarbons in this oil have not the composition of the series C_nH_{2n+2} which Pelouze and Cahours deduced from their analyses, but, as we have found by results not yet published, the composition of the series C_nH_{2n}. Pentane, hexane, heptane and octane had been identified by Schorlemmer, and the classic work of C. M.

Warren had shown the existence in Pennsylvania petroleum of the two series of isomeric hydrocarbons from pentane to octane. The large deposits of petroleum in northern Ohio and Indiana, Texas, Colorado, Wyoming and Kansas had not then been discovered.

In 1885, soon after the first well was drilled that yielded oil from the Trenton limestone, two oil inspectors brought me a five-gallon can of Trenton limestone oil and remained while I examined it for them. This was my first acquaintance with the sulphur petroleums. I recognized at once the large percentage of sulphur and soon afterward began a study of the sulphur compounds. I am not now fully satisfied as to the nature of those sulphur compounds. Not long afterward I also procured the Canadian sulphur oil, and carried along together the study of these products, the one from Trenton limestone and the other from the Canadian Corniferous limestone. The composition of Ohio oil has only recently been determined, with respect to the principal series of hydrocarbons, by a research completed during the present month, seven years after it was begun. At first I had no preconceived ideas as to the series of hydrocarbons which compose these crude oils, except what knowledge I had gathered from the work of my predecessors; but after the work had progressed far enough to see that the crude oils from the different fields were essentially different in certain constituents, especially in sulphur, I was inclined to look on the Trenton and Corniferous limestone crude oils as a special species, the sulphur petroleums, and to agree with Peckham in his specific classification of the different petroleums as varieties of bitumens. But I soon became convinced that no such sharp distinctions based on composition could be drawn. Now after these years of arduous labor I have reached the conclusion that petroleum from whatever source is one and the same substance, capable of a simple definition—a mixture in variable proportions of a few series of hydrocarbons, the product of any particular field differing from that of any other only in the proportion of these series and the members of the series. I arrived at this conclusion only one year ago, when it was found that the higher distillates from Pennsylvania petroleum contain the series C_nH_{2n-2}, which until then I had supposed was only to be found in the heavier California and Texas oils, or the so-called asphaltic oils. Results obtained within the last two months show that Ohio petroleum has a similar composition.

In support of this definition I would suggest that, so far as known,

all petroleum contains nitrogen and sulphur, although the proportion of nitrogen in Pennsylvania and Ohio crude oils is much smaller than that in California oil, and that the percentage of sulphur is much smaller in the Pennsylvania sandstone oils—only a trace as compared with the larger amounts in Trenton limestone, Corniferous limestone, in California and Texas oils. With this definition the distinction drawn, at least in commercial circles, between paraffine and asphaltic oils disappears, for Pennsylvania crude oil contains the asphaltic hydrocarbons, although I cannot assert that California oil contains paraffine. I have crystalline hydrocarbons separated from California oil, but their identity is not yet fully established. The refiner is more definite in his classification; he knows from experience that the best yield of gasoline is from Pennsylvania oil, and none from California oil. He is fully aware that it is useless to expect to obtain a respectable yield of burning oil from California or Texas petroleum, and that he cannot hope to obtain paraffine from those heavy oils. But his very heavy lubricating oils and heavy pitches and asphalts he knows can come only from the heavier petroleums.

With reference to a nomenclature of the petroleum series and hydrocarbons, no system can safely be adopted until the structure of these bodies is better understood. The aromatic hydrocarbons benzol and its homologues are present in all petroleum so far as examined, but in widely variable proportions. Pennsylvania crude oil contains the lower members in small amounts, but not the higher homologues. It is true that anthracene and its congeners have been described as separated from petroleum residues, but it is probable that such bodies are not present in the original oil; they are doubtless formed by decomposition during distillation. California petroleum contains much larger proportions of the aromatic hydrocarbons, especially of the xylols and others with higher boiling points. In one of our distillates from California crude oil so much naphthaline was present that the distillate became solid on slight cooling. This distillate came over at about 215°. The only other instance in which naphthaline has been found in petroleum was its separation by Warren and Storer from Rangoon petroleum.

The terminology of the series C_nH_{2n+2} has been well defined, and the names adopted by Kraft for the members with high boiling points, liquids and solids, which he separated from shale distillates are applicable to the corresponding bodies in Pennsylvania crude

oil. It is interesting to note that Pennsylvania petroleum alone, unless we include the analogous Berea Grit and other similar sandstone oils of southern Ohio and Virginia, contains the unbroken series up to and including the solid paraffine constituents. Although the Ohio Trenton limestone oil and the Canadian Corniferous oil contain paraffine, the former in large proportions, the liquid members of series C_nH_{2n+2} stops with $C_{11}H_{22}$ in both Canadian and Ohio oil. The liquid hydrocarbons from there on, so far as examined, are members of series poorer in hydrogen.

The series C_nH_{2n} has been variously named. When first discovered in petroleum, and the hydrocarbons found to be identical with the hydrogen addition products of benzol and its homologues, the hydrocarbons from petroleum were described as hexahydro-bodies. On the discovery of a long series of these hydrocarbons in Russian oil, Markownikow suggested the name naphthenes. But when later the origin and nature of the methylenes were better understood and cyclic hydrocarbons found in petroleum identical with the synthetic products, the name methylene was adopted for the lower petroleum hydrocarbons. These closed-chain hydrocarbons differ in their deportment toward reagents from those with an open-chain, C_nH_{2n+2}. While it is to be assumed that the series C_nH_{2n} is represented in its higher members by the methylenes, some extension of the nomenclature is necessary to include those bodies. There can evidently be but one ring with these proportions of carbon and hydrogen. For instance, the hydrocarbon $C_{12}H_{24}$ must be regarded as a long chain with the ends connected, dodecamethylene, or a lower ring with several side chains.

When the series C_nH_{2n-2} is reached it becomes necessary to assume a union of two rings attached by one carbon atom in each ring. In the series C_nH_{2n-4} the union would be between two carbon atoms in each ring and the members should include, for instance, octohydronaphthaline which would represent the hydrocarbon $C_{10}H_{16}$. In the line of this suggestion, following the analogy of naphthaline, one side chain should be capable of oxidation giving a derivative of phthalic acid. But the methylene hydrocarbons seem to possess a different order of stability toward the action of reagents, and we have observed this peculiarity in bodies separated from petroleum which appear to be derivatives of the methylenes. For instance, the nitrogen compounds separated from California crude oil cannot be oxidized into closely allied products, the

oxidation always proceeding to the formation of ultimate products, nitrogen and CO_2, and we have found it impossible to check it.

The same is true of the sulphur compounds, which also appear to be methylene derivatives. These bodies oxidize with the greatest ease to sulphuric acid, and it is difficult to control the oxidation. We have done this, however, and have obtained well-defined sulphones. In general terms, the addition of hydrogen to benzol and its homologues weakens the resistive action toward reagents. This difference in stability between the series C_nH_{2n+2} and the series poorer in hydrogen appears in commercial use of the heavier products. We have recently compared the flashing point and fire test to heavy distillates from Pennsylvania crude oil and crude oils from California, Texas, etc., and it appears that the products from Pennsylvania oil have higher flashing points and fire tests than those from other fields. We have an excellent opportunity to ascertain the general application of this observation, for we have at hand samples in gallon lots of the principal lubricators made from Pennsylvania and Ohio oils on the market, and also samples of crude oils from the various fields; for example, two barrels of crude oil from Baku in the Russian field.

Products are now being prepared from the crude oils to compare with Pennsylvania lubricating oils. The inferior stability of petroleum which the series C_nH_{2n} or series poorer in hydrogen predominates has appeared in all our work on the various crude oils. For instance, the admission of air into hot Pennsylvania distillates never causes an explosion; but explosions are sure to follow the contact of air with hot distillates from other fields where the principal series is lower than the series C_nH_{2n+2}.

In combustion it is easy to see the difference in stability, in the readier separation of carbon. Then in analysis of a series, say from $C_{12}H_{24}$ to $C_{21}H_{44}$, in the lower members no carbon separates in the boat, but it gradually appears with increasing molecular weight, in larger and larger amounts.

The greater stability of the hydrocarbons C_nH_{2n+2} doubtless explains the superior quality of burning oils prepared from Pennsylvania petroleum, together with the fact of a larger proportion of hydrogen. The series poorer in hydrogen more readily separates carbon as soot and is more difficult to burn. A mixture of the two series C_nH_{2n+2} and C_nH_{2n} forms a good burning oil and probably accounts for the excellent quality of Russian burning oil.

As I shall presently explain, Ohio crude oil contains a much smaller proportion of the series C_nH_{2n+2}, and it cannot be expected to yield burning oil of as good quality as Pennsylvania crude oil. Canadian burning oil should be still poorer in quality, which is easily seen by the consumer who will pay a high duty on United States oil rather than use his own product.

Pennsylvania Petroleum.

As mentioned above, Pennsylvania crude oil contains minute amounts of sulphur and nitrogen compounds, and a small proportion of benzol derivatives; but the great bulk of the oil is composed of the series C_nH_{2n+2}, beginning with the butanes and ending with solid hydrocarbons of such high molecular weights that they cannot be determined by any method now known. We have reached the hydrocarbon $C_{28}H_{58}$, and it is the last one of the series whose molecular weight could be determined. It is quite probable that there are as many as eight or even more of the hydrocarbons with greater molecular weight.

It has been an open question with practical oil men, and perhaps is still with some, as to whether solid paraffine is contained in crude petroleum or whether it is formed in the process of distillation. In the ordinary process of refining crude oil, a very considerable proportion of the hydrocarbons is lost in the last stages of the destructive distillation which ends with a large mass of coke. On comparing the thin liquid crude oil with products obtained from it in refining, it would be natural for the superficial observer to reason, from the appearance of coke at the end of the distillation and other heavy products, that solid paraffine should be formed in a similar way by decomposition of the light liquid crude oil.

But careful consideration of the nature and origin of petroleum precludes the possibility of its formation by distillation. It is true that paraffine was first obtained by Reichenbach by the distillation of vegetable and animal organic matter, and there is no question that it has been formed in a similar manner by natural processes. But petroleum must be regarded as a final product of decomposition, and while the series may be changed from one to another to a limited extent, decomposition of the constituents leads to the formation of simpler products until finally carbon is reached. Therefore, instead of paraffine as a result of decomposition of other

hydrocarbons, paraffine itself is decomposed into hydrocarbons of lower molecular weight. But while this view is well supported by the facts observed relating to the nature of paraffine, we have not been satisfied with less than actual proof by experiment of its presence in crude oil, and several lines of work have been carried on in this direction.

Last year we placed several liters of crude petroleum in a large flue of the laboratory, with strong draught, and allowed it to evaporate during several weeks. Much the larger portion of the original oil had evaporated, and the residue was so very thick it would scarcely flow. By careful extraction of the oil with ether and alcohol we obtained a small amount of solid paraffine, as shown by its melting-point and resemblance to ordinary solid paraffine hydrocarbons.

In another line of work, the results of which have not been published, we procured ten gallons of a semi-solid mass of hydrocarbons from a refining company at Coreopolis, Pa., that had been collected from the sucker rods in pumping oil and had never been distilled; this oil is very heavy, light yellow in color and is used for the preparation of commercial cosmolines and vaselines. By cooling some of this product and crystallization we were able to separate from it a mixture of hydrocarbons closely resembling paraffine. A considerable portion of this pasty mass was subjected to fractional distillation, and a series of hydrocarbons separated with the composition of the series C_nH_{2n+2}. The oils separated by cooling and pressure gave results on analysis corresponding to series poorer in hydrogen.

We next took up the composition of the mixtures that form the vaselines and cosmolines. The refiner makes a sharp distinction between crystallizable and uncrystallizable paraffine. But there seems to be but one form of solid paraffine hydrocarbons. Vaseline is simply a very heavy oil saturated with paraffine, and containing an excess of solid paraffine in the form of an emulsion. The oil is composed of the heavy oils of the series C_nH_{2n} and C_nH_{2n+2}, and the solid bodies members of the series C_nH_{2n+2}. The so-called scale paraffine of the refiner is solid paraffine containing sufficient of the heavy oils to prevent it from assuming a well-defined crystalline condition.

The appearance of the series C_nH_{2n} and the series C_nH_{2n-2} in Pennsylvania petroleum distillates, as shown in a paper published

last year, indicates that the so-called asphaltic hydrocarbons form a part of this petroleum with very high boiling points. This places Pennsylvania petroleum in the same category with the heavier petroleum from such fields as California and Texas, the chief differences being the predominating series C_nH_{2n+2} in Pennsylvania oil and the series poorer in hydrogen in the heavier products. As explained above, the large proportion of the paraffine hydrocarbons in the heavy portions of Pennsylvania oil apparently render the lubricating distillates more stable. Just what effect it has on the lubricating qualities, so far as I know, has not been completely determined. Some experiments on the very heavy lubricants from Beaumont oil have demonstrated very superior lubricating qualities.

Ohio Trenton Limestone Petroleum.

Since the first discovery of this petroleum, there has been great uncertainty concerning its composition. In the preparation of commercial products, the refiner discovered essential differences between it and Pennsylvania oil which were fully understood with reference to its refining qualities. The first serious obstacle was the large amount of sulphur compounds that must be removed for the production of acceptable burning oil. Innumerable patents were issued for processes which included distillation over quartz, precipitation with mercuric chloride, oxidation with potassium permanganate, and numerous other impracticable ideas that had been tried only on paper. The ordinary refiner distils over scrap iron and refines with alkaline lead oxide. From much the greater part of refining oil sulphur is removed by distilling over heated copper oxide and recovery of the oxide, a process that is said to have originated in Canada, but is known as the Frasch process. Probably fifty tons of sulphur daily is a conservative estimate of the amount extracted from Ohio oil and burned off into the atmosphere. It is claimed for this process that it is capable of removing the sulphur to 0.02 per cent., which is probably correct. Excellent burning oils are made from Ohio petroleum.

The composition of Ohio petroleum, so far as the portions readily distilled are concerned, has only been arrived at within the last few months. Several years ago an examination of the sulphur petroleums, as Ohio and Canadian petroleum was then designated, showed that the series C_nH_{2n+2} formed the portions of Ohio crude

oil which distilled below 212°, and the same members were discovered that had been previously identified in Pennsylvania oil, although the proportion of these hydrocarbons was smaller than in Pennsylvania oil. The proportion of aromatic hydrocarbons is higher in Ohio than in Pennsylvania oil. The lower methylenes are also probably contained in larger proportion in the Ohio oil. An investigation just finished on the hydrocarbons contained in the limits between 112° and 280°, tension 30 mm., has identified thirteen hydrocarbons, with very satisfactory data on the proportions of carbon and hydrogen which establish the series, and the molecular weights and indices of refraction which identify the members of the series. The following hydrocarbons of the series C_nH_{2n} were found: $C_{12}H_{24}$, $C_{13}H_{26}$, $C_{14}H_{28}$, $C_{15}H_{30}$, $C_{16}H_{32}$, $C_{17}H_{34}$. Unfortunately the distillate that should yield the hydrocarbon $C_{18}H_{36}$ was lost, although its specific gravity was ascertained before filtration.

Of the series C_nH_{2n-2}, the following hydrocarbons were identified: $C_{19}H_{36}$, $C_{20}H_{38}$, $C_{21}H_{40}$, $C_{22}H_{42}$; and of the series C_nH_{2n-4}, the hydrocarbons $C_{23}H_{42}$, $C_{24}H_{44}$, $C_{25}H_{46}$.

The change in series is attended with a greater difference in specific gravity between adjacent hydrocarbons; for instance, the last change in series is very marked, as the following table shows:

Distillate.	*Sp. Gr.*	*Hydrocarbon.*	*Difference in Sp. Gr.*
224–227°	.8614	$C_{21}H_{40}$	
237–240°	.8639	$C_{22}H_{42}$	.0025
253–255°	.8842	$C_{23}H_{42}$	.0225
263–265°	.8864	$C_{25}H_{48}$	.0022

The distillates from which these hydrocarbons were separated above 150°, 30 mm., contained a large proportion of solid paraffine. The higher fractions were solid at ordinary temperatures, but no attention was given to the solid constituents, for without doubt they are identical with the solid hydrocarbons identified in Pennsylvania oil. Much difficulty was met with in separating the liquid constituents. The distillate was first cooled to 0° and filtered, and the filtrate then cooled to —10° and again filtered.

In purifying these heavy oils they were first dissolved in gasoline, and after purification the gasoline was removed by distillation. The greater preponderance of the series poorer in hydrogen in Ohio oil over Pennsylvania oil explains the higher specific gravity

of Ohio crude oil. The series C_nH_{2n-4} does not appear in Pennsylvania oil within the range of distillates below 300°, but it does appear in Ohio oil. The proportions of the series still poorer in hydrogen in the residues of distillation from Ohio oil are doubtless still greater.

Canadian Corniferous Limestone Petroleum.

In the paper referred to above, the composition of the distillates from Canadian oil was explained, including the hydrocarbons $C_{11}H_{22}$ and $C_{12}H_{24}$, which were identified in the fractions 196° and 214°. This limits the series C_nH_{2n+2} in Canadian oil to the lower members. Two years ago the higher fractions were examined for the individual hydrocarbons and results obtained, not yet published, that show a continuation of the series C_nH_{2n}; and the hydrocarbons separated included the following: $C_{13}H_{24}$, $C_{13}H_{26}$, $C_{14}H_{28}$, $C_{15}H_{30}$, $C_{16}H_{32}$.

These bodies were identified by combustion for the series, and their molecular weights ascertained for the individual members of the series; the specific gravity of each hydrocarbon agrees closely with that of the corresponding hydrocarbon of Ohio petroleum. These values were still further confirmed by the formation and analysis of the chlorides.

The proportions of the lower members of the series C_nH_{2n+2}, which form the naphtha and gasoline in Canadian petroleum, is considerably smaller than in Ohio petroleum. The proportion of burning oil distillates is also less, and it is not possible to make from Canadian oil so good burning oil. The series C_nH_{2n} shows less stability on standing than the higher series C_nH_{2n+2}. I have samples of burning oil from Canadian petroleum that have stood ten years; they have changed from "water white," the original quality of the oil, to very dark yellow. Much larger quantities of gas are evolved in refining the Canadian oil, which is run back for heating the stills. Some paraffine is made, but the yield is small. The sulphur in the crude oil gives much trouble in refining, and it is not all removed in the burning oil. The percentage of sulphur is higher than in Ohio petroleum as a rule, usually about one per cent. Canadian petroleum should give good grades of lubricators, but I have never examined these products.

California Petroleum.

In a paper published two years ago the composition of California petroleum oil from different sections of those fields was explained, and the principal series in the range of distillates examined, which included those below 214° in all specimens of crude oils, showed the series C_nH_{2n}.

Allusion was made in the former publication to a specimen of exceptionally heavy oil from Summerland, Santa Barbara county, of especial interest, since it came from wells sunk below the level of the Pacific Ocean at high tide. No distillates were collected from this oil below 200° atmospheric pressure. Under a tension of 60 mm. continued fractional distillation separated very heavy oils that were colorless or slightly yellow. They were purified by dissolving in gasoline and agitating with sulphuric acid, common and fuming, and the gasoline distilled off with the aid of a current of carbonic dioxide. The composition of these products proved to be very different from that of the other California oils, or from any others we have examined. For instance, the fraction 210°–215°, 60 mm., gave as its specific gravity at 20°, 0.9085, and the proportions of carbon and hydrogen corresponded to the hydrocarbon $C_{19}H_{34}$, or the series C_nH_{2n-4}. The higher members were the most viscous distillates that we have separated in what appears to be a pure form from any petroleum.

So far as we have carried the examination of California petroleum, no solid paraffine hydrocarbons have been found. From several fields oil has been obtained whose higher distillates on standing deposited large well-defined crystals, but unlike paraffine. From the fractions between 275° and 295°, 60 mm., separated from Torrey cañon oil, a considerable quantity of crystals separated on standing several months that melted at 57° to 62°. These crystals were readily soluble in benzol and alcohol, and crystallized from hot alcohol on cooling apparently in a pure form, unlike the solid paraffine hydrocarbons that are very sparingly soluble in alcohol; sufficient of this product for complete identification has not yet been obtained. The higher portions of heavy California petroleum offer an attractive field for study of the series poorer in hydrogen.

Texas Petroleum.

Much attention has been attracted to the recent discoveries of oil in Texas, and in some respects these deposits of oil possess a

peculiar interest. The older Corsicana field yields an oil that is adapted for the preparation of a fairly good grade of burning oil, but it is inferior to Pennsylvania oil, since, as Richardson has shown, it is composed chiefly of the methylene hydrocarbons. The heavier oil at Beaumont does not yield a sufficient proportion of burning oil distillate to make its preparation economical, but it is stated that a distillate can be separated in small quantities without cracking that can be refined into an inferior grade of burning oil. So far as examined the Beaumont oil does not contain members of the series C_nH_{2n+2}, which is essential in oils that yield the best grades of kerosene. The unique occurrence of this crude oil, underlying beds of sulphur under rather loose beds of shale, should exclude any of the most volatile constituents, such as are found in Pennsylvania oil. As we have demonstrated, the predominating series of hydrocarbons include the methylenes and condensed series. The crude oil is very heavy; it easily decomposes under distillation, but by exclusion of air very heavy distillates may be separated without decomposition, from which superior lubricating oils may be prepared, especially of the heaviest type. The heaviest residue from Beaumont oil, if decomposition has been prevented, forms the best sort of petroleum asphalt, much heavier than similar products to be obtained from any other than California crude oil; in fact all the products to be obtained from Texas oil resemble those prepared from California oil.

The sulphur compounds in Texas oil seem to be much less stable than those in Ohio and Canadian oils, perhaps on account of their higher molecular weight. It is worthy of note that heavy petroleum, such as that from Texas and California fields, contain more sulphur than more volatile crude oils, like the Pennsylvania.

Petroleum from other fields, such as Colorado, Wyoming, Japan and South America, all partake of the properties of the heavier products from the fields in this country. The Japanese crude oils were very carefully sampled for our examination three years ago. There is promise of a great development of oil territory in South America. I scarcely believe that the sample of heavy oil we examined some years ago represents the true condition of the oil fields there. The heavy petroleums are rapidly increasing in value as fuel. Prices have recently been advanced in Texas to seventy-five cents per barrel.

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I have a large amount of unpublished data on the sulphur and nitrogen compounds in petroleum. Although I have had the sulphur compounds under examination for nineteen years, I am not yet sure as to the form of the higher series. In a paper presented to the New York Section of the Society of Chemical Industry two years ago, and published in the *Society Journal*, a brief account of the sulphur and nitrogen compounds was given. It was explained that these bodies are members of a series $C_nH_{2n}S$, and that they oxidize into sulphones and very readily into sulphuric acid. These bodies are doubtless ring compounds, as was then suggested, similar to the thiophenes.

California petroleum contains a larger proportion of nitrogen base than any other, so far as known. Two per cent. of nitrogen, the amount contained in several specimens of crude oils examined, corresponds to twenty or twenty-five per cent. of the basic oils, or about one-quarter of the crude oil consists of the nitrogen compounds. In structure these bodies are tetra- or octohydro-ring compounds in homologous series. The tetrahydro-condition is shown by their instability.

It is, therefore, apparent that a similar condition of instability prevails in the methylenes, and in the sulphur and nitrogen compounds from heavy petroleum. The sulphur and nitrogen bodies are found in considerable quantities only in such petroleum as is mainly composed of the methylenes or series poorer in hydrogen.

Another interesting series of bodies found in California, but not in Eastern oils, at least to the same extent, are the phenols, which are present in considerable quantities in some of the California oil.

Natural Formation of Petroleum.

Much as has been said on this attractive subject, a broader knowledge of facts is necessary before definite conclusions can be reached. What is known forms the basis for only one explanation concerning the formation of petroleum, and that is that it was formed from vegetable or animal matter by slow decay or breaking down from the complex forms of vegetable or animal life under the influence of natural forces, with no great elevation in temperature such as is necessary for distillation.

Mendelejeff's theory of the formation from carbides at high temperatures, recently asserted with greater force on the basis of

Moissan's work with the electric furnace, demands too many hypothetical assumptions, and it has too little support on the basis of fact. To reason from the artificial formation of alloys and carbides in an electric furnace to the natural formation of petroleum containing nitrogen, sulphur and oxygen, in the form of hydrothophenes, hydrochinolines, and phenols, demands a too broad reach of the imagination to make the connections.

Bearing in mind the fact that petroleum may now be regarded as one and the same substance whatever its source, and that the deposits in different fields are composed of the same series, differing only in the proportions of these constituents, it must be admitted that it had one origin and one only. With reference to the series of hydrocarbons, it is immaterial whether its source was animal or vegetable, for under the influence of natural agencies it could have been formed as well from one as from the other.

This question has been attacked on chemical grounds from the wrong direction. Because hydrocarbons of the marsh gas series, ethylene series or acetylene series at temperatures of decomposition form minute quantities of the aromatic series, or that hexahydroaromatic bodies are formed from the aromatic hydrocarbons by heating with hydriodic acid, to assume that these same changes were produced by natural agencies and resulted in the formation of the hydrocarbons which now constitute petroleum, together with the other constituents of petroleum, ascribes to these natural agencies a direction of action and power that we do not know they possess.

In considering present knowledge with reference to the natural formation of petroleum, it seems to me that the following questions must be answered:

1. What is the chronology of petroleum: in what order were the deposits formed in different fields?

2. Were the least volatile constituents formed from the most volatile or the reverse?

3. What is a reasonable explanation of the formation of the other constituents of petroleum?

The first question must be answered by the geologist.

It is natural to assume that the limestones formed by the accumulation of the shell remains of animal life were deposited first from the ancient sea. The sandstones, as products of erosion from the older rocks, were deposited last. The question as to whether the

different deposits of petroleum were formed *in situ*, or formed in other strata and by some natural agency transferred to their present location, has not I believe been satisfactorily answered by the geologists. In the case of the limestone petroleum, it would seem that it must have been formed where it is now to be found, as Hunt and Orton have ably maintained.

The theory of distillation from some other strata is not tenable in the light of present knowledge of the constituents of petroleum. Neither could any known constituents of plants that could form petroleum be distilled, nor could the heavier portions of petroleum be distilled; the result would be only very volatile distillates and deposits of coal or graphite. In this condition deposits of petroleum should always be accompanied by coal, or with coal in the near vicinity.

In the case of Pennsylvania and the allied southern Ohio and West Virginia petroleum, it would be a great discovery to connect these deposits with the coal formations, for then the source would unquestionably be vegetable growth and would support the prevailing opinion that this was the source of petroleum of this class. It is reasonable to assume, as is now believed, that Pennsylvania oil was not formed in the sandstones, but found its way there by natural agencies from lower strata, probably the Devonian shales. The infiltration of the crude oil through sandstones would have a purifying effect. It is quite probable that the very light yellow crude oils from the Berea Grit and other sandstones were filtered a second time or more into their present positions.

With reference to the source of the limestone oils, the evidence is all in favor of animal origin, and the same is true of California oil, although its formation is probably far more recent than that of the others. Texas petroleum has not been sufficiently studied in relation to its occurrence and composition, but it is evidently of more recent origin, like California oil.

With reference to the second question, is it more reasonable to assume, for instance, that the solid paraffine hydrocarbons were formed from the lower members of this series, or that the lower members were formed from paraffine? On this point some experimental evidence may be brought to bear. Reichenbach obtained paraffine from both vegetable and animal organic matter. Engler obtained paraffine by the distillation of fish oil, as Warren and Storer had done many years previously.

It is well known that paraffine breaks down very readily into hydrocarbons with lower molecular weights, but it is not possible to polymerize the lower hydrocarbons into the solid paraffine hydrocarbons. The tendency in cracking of any constituents of petroleum is toward the formation of the lower series and finally carbon in the form of coke. So far as experimental evidence and observation have shown the nature and relations of the hydrocarbons which compose the different series in petroleum, the conclusion is convincing that the lower members of the series were formed from the higher. A single break in the ring of a methylene is sufficient to form by the addition of hydrogen a paraffine hydrocarbon.

In answer to the third question, as to the formation of the sulphur, nitrogen and oxygen compounds in petroleum, these bodies have evidently not been built up synthetically, but are the products of decomposition of more highly organized constituents of organic bodies. It would seem that the small proportions of these bodies in Pennsylvania oil, as compared with the larger proportions in the limestone oils and California oil, should be strong evidence in favor of a different origin, that Pennsylvania oil came from organic vegetable remains, which should permit of the small amounts of sulphur and nitrogen compounds found in this class of oils.

But I think it can be asserted as a fact that the very large proportion of nitrogen compounds in California petroleum, amounting to one-fifth or more of the total weight of the oil, can only be accounted for by accepting animal remains as the source of their formation. As a summation of what is at present known of the origin of petroleum, the following answers may be given to the questions propounded above:

1. Petroleum containing large proportions of the volatile hydrocarbons, especially of the series C_nH_{2n+2}, such as Pennsylvania petroleum, was formed from vegetable organic matter. The limestone petroleum and California petroleum was formed from organic matter of animal origin.

2. Cellulose, starch and other similar bodies in plants, and the fats and nitrogen compounds in animal bodies, by gradual decomposition with exclusion of air, gave first the heavier bodies found in petroleum, and by natural agencies during long periods of time, with no considerable rise in temperature, further decomposition included as products the hydrocarbons with smaller molecular weights.

3. The nitrogen and sulphur constituents of petroleum could only have been formed directly from or through the agency of animal organic matter.

There is an attractive field for the chemical geologist to study, more intimately than has ever been done, the occurrence of petroleum in connection with its composition.

CLEVELAND, O.

THE FORWARD MOVEMENT IN PLANT-BREEDING.

BY L. H. BAILEY.

(*Read April 2, 1903.*)

The first specific interest in cultivated plants was in the gross kinds or species. As the contact with plants became more intimate, various indefinite form-groups were recognized within the limits of the species. Gradually, with the intensifying of domestication and cultivation, very particular groups appeared and were recognized. These smaller groups came finally to be designated by names, and the idea of the definite and homogeneous cultural variety came into existence. The variety-conception is really a late one in the development of the human race. It is practically only within the past two centuries that cultivated varieties of plants have been recognized as being worthy of receiving designative names. It is within this period, also, that most of the great breeds of animals have been defined and separately named.

All this measures the increasing intimacy of our contact with domesticated plants and animals. It is a record of our progress. The peoples that are most advanced in the cultivation of any plant are the ones that have the most named varieties of that plant. In Japan, to this day, the plums pass under ill-defined class-names. We have introduced these classes, have sorted out the particular forms that promise to be of value to us and have given them specific American names. Not long ago a native professor in Japan wrote me asking for cions of these plums, in order that he might introduce Japanese plums into Japan. The Russian apples are designated to some extent by class-names; in fact, it was not until the appearance of Regel's work, about a generation ago, that Russian pomology may be said to have been born. What

constitutes a variety is increasingly more difficult to define, because we are constantly differentiating on smaller points. The growth of the variety-conception is really the growth of the power of analysis.

The earlier recognized varieties seem to have come into existence unchallenged. There is very little record of inquiry as to how or why or even where they originated. That is, the quest of the origin arose long after the recognition of the variety as a variety. Even after inquisitive search into origins had begun there was little effort to produce these varieties. The describing of varieties and the search into their histories was a special work of the nineteenth century. One has only to consult such American works as Downing's *Fruits and Fruit Trees of America* and Burr's *Field and Garden Vegetables of America*, to see how carefully and methodically the descriptions and synonymy of the varieties were worked out. These are types of excellent pieces of editorial and formal systematic work.

There have been isolated efforts at producing varieties for many years. These efforts began before the time of the general discussion of organic evolution. In fact, it was on such experiments that Darwin drew heavily in some of his most important writings. Roughly speaking, however, the conception that the kinds of plants can be definitely modified and varied by man is a product of the last half century. We now believe that there is such a possibility as plant-breeding. It is really a more modern conception, so far as its general acceptance is concerned, than animal-breeding. But both animal-breeding and plant-breeding are the results of a new attitude toward the forms of life—a conviction that the very structure, habits and attributes are amenable to change and control by man. This is really one of the great new attitudes of the modern world.

Formerly, and even up to the present time, the variety has been taken as the unit for plant-breeding work, as it has been for descriptive and classificatory work. Whether we believed it or not, we have accepted it as a fairly definite thing or entity. Yet, what is a variety? Only the ideal of one man or a set of men. Custom may define its boundaries, but in fact it has no boundaries. At best, a variety is only an assemblage of forms that agree rather more than they differ: and any one of these forms may, with equal propriety, be called another variety. Shall we continue to

consider the variety as a unit or basis from which we are to breed for the purpose of producing other varieties? Or shall we still further refine our ideals and find that the variety-conception is really only a mark of an imperfect and superficial development of an immature age?

Now, plant-breeding is worthy of the name only as it sets definite ideals and is able to attain them. Merely to produce new things is of no merit: that was done long before man was evolved. A child can "produce" a new variety, but it may learn nothing and contribute nothing in producing it. I have myself produced 1500 new kinds of pumpkins and squashes, but I had no idea what I was to produce, the world is no better for my having produced them, and I am no wiser (except in experience) than I was before. In many "new" things that are produced, there may be dispute as to whether they are new and as to whether they are distinct enough to be named and therefore to be ranked as varieties at all. This is not science, nor even breeding: it is playing and guessing. What does the world care whether John Jones produces "Jones' Giant Beardless wheat"? But it does care if he produces a wheat having a half of one per cent. more protein. We must give up the production of mere "varieties"; we must breed for certain definite attributes that will make the new generations of plants more efficient for certain purposes: this is the new outlook in plant-breeding.

Happily, we are not without abundant accomplishment in this new field. The last ten years has seen a remarkable specialization in the producing of plants that are adapted to particular needs. The days of merely crossing and sowing the seeds to see what will turn up are already past with those who are engaged seriously in the work. The old method was hit-and-miss and the result was to take what good luck put in your way: the new method proceeds definitely and directly and the result is the necessary outcome of the line of effort. The crux of the new ideal is efficiency in one particular attribute in the product of the breeding. These attributes are measurable: the kind of results are foreseen in the plan, or are predictable.

All these remarks are typically illustrated in the experiments with corn-breeding conducted in Illinois. It is significant to note what are the reasons for breeding new corns, as stated by Professor Hopkins in Bulletin 82 of the Illinois Experiment Station:

"In its own publication a large commercial concern, which uses enormous quantities of corn, makes the following statements:

"'A bushel of ordinary corn, weighing fifty-six pounds, contains about four and one-half pounds of germ, thirty-six pounds of dry starch, seven pounds of gluten, and five pounds of bran or hull, the balance in weight being made up of water, soluble matter, etc. The value of the germ lies in the fact that it contains over forty per cent. of corn oil, worth, say, five cents per pound, while the starch is worth one and one-half cents, the gluten one cent, and the hull about one-half cent per pound.

"'It can readily be seen that a variety of corn containing, say, one pound more oil per bushel would be in large demand.

"'Farmers throughout the country do well to communicate with their respective agricultural experiment stations and secure their co-operation along these lines.'

"These are statements and suggestions which should, and do, attract the attention of experiment station men. They are made by the Glucose Sugar Refining Company of Chicago, a company which purchases and uses, in its six factories, about fifty million bushels of corn annually. According to these statements, if the oil of corn could be increased one pound per bushel, the actual value of the corn for glucose factories would be increased five cents per bushel; and the president of the Glucose Sugar Refining Company has personally assured the writer that his company would be glad to pay a higher price for high oil corn whenever it can be furnished in large quantities. The increase of five cents per bushel on fifty million bushels would add $2,500,000 to the value of the corn purchased by this one company each year. The glucose factories are now extracting the oil from all the corn they use and are unable to supply the market demand for corn oil. On the other hand, to these manufacturers protein is a cheap by-product and consequently they want less protein in corn.

"Corn with a lower oil content is desired as a feed for bacon hogs, especially for our export trade, very extensive and thorough investigations conducted in Germany and Canada having proved conclusively that ordinary corn contains too much oil for the production of the hard firm bacon which is demanded in the markets of Great Britain and Continental Europe."

It is very interesting to note that this does not mention the improvement of Leaming's White, or Jones' Yellow Dent, or any

other named variety of corn, nor does it propose that any new variety shall be created. It suggests what may be done with any variety of corn. The experiments in Illinois demonstrate that "the yield of corn can be increased, and the chemical composition of the kernel can be changed as may be desired, either to increase or to decrease the protein, the oil, or the starch."

The breeding of the corn proceeds along two general lines—for physical perfection and for chemical perfection. Selection for physical merit proceeds as follows, to quote again from Professor Hopkins: "The most perfect ears obtainable of the variety of corn which it is desired to breed should be selected. These ears should conform to the desirable standards of this variety and should possess the principal properties which belong to perfect ears of corn, so far as they are known and as completely as it is possible to secure them. These physical characteristics and properties include the length, circumference, and shape of the ear and of the cob; the number of rows of kernels and the number of kernels in the row; the weight and color of the grain and of the cob; and the size and shape of the kernels. In making this selection the breeder may have in his mind a perfect ear of corn and make the physical selection of seed ears by simple inspection, or he may make absolute counts and measurements and reduce the physical selection almost to an exact or mathematical basis."

The selection for chemical content is made on two bases—on the general gross structure of the corn kernel as determined by "mechanical examination," and on chemical analysis of the kernel.

Chemical examination by means of mechanical examination is as follows:

"The selection of seed ears for improved chemical composition by mechanical examination of the kernels is not only of much assistance to the chemist in enabling him to reduce greatly the chemical work involved in seed corn selection, but it is of the greatest practical value to the ordinary seed corn grower who is trying to improve his seed corn with very limited service, if any, from the analytical chemist. This chemical selection of seed ears by mechanical examination, as well as by chemical analysis (which is described below), is based upon two facts:

"1. That the ear of corn is approximately uniform throughout in the chemical composition of its kernels.

"2. That there is a wide variation in the chemical composition of different ears, even of the same variety of corn. These two facts are well illustrated in Table 1.

TABLE I. PROTEIN IN SINGLE KERNELS.

		Ear A, protein, per cent.	*Ear B, protein, per cent.*	*Ear C, protein, per cent.*	*Ear D, protein, per cent.*
Kernel	No. 1	12.46	11.53	7.45	8.72
"	" 2	12.54	12.32	7.54	8.41
"	" 3	12.44	12.19	7.69	8.73
"	" 4	12.50	12.54	7.47	8.31
"	" 5	12.30	12.14	7.74	9.02
"	" 6	12.49	12.95	8.70	8.76
"	" 7	12.50	12.84	8.46	8.89
"	" 8	12.14	[1]	8.69	9.02
"	" 9	12.14	12.04	8.86	8.96
"	" 10	12.71	12.75	8.10	8.89

"It will be observed that while there are, of course, small differences among the different kernels of the same ear, yet each ear has an individuality as a whole, the difference in composition between different ears being much more marked than between different kernels of the same ear.

"The uniformity of the individual ear makes it possible to estimate or to determine the composition of the corn by the examination or analysis of a few kernels. The remainder of the kernels on the ear may then be planted if desired. The wide variation in the composition between different ears furnishes a starting-point for the selection of seed in any of the several different lines of desired improvement.

"The methods of making a chemical selection of ears of seed corn by a simple mechanical examination of the kernels is based upon the fact that the kernel of corn is not homogeneous in structure, but consists of several distinct and readily observable parts of markedly different chemical composition (see illustrations). Aside from the hull which surrounds the kernel, there are three principal parts in a grain of corn:

"1. The darker colored and rather hard and horny layer lying next to the hull, principally in the edges and toward the tip end of the kernel, where it is about three millimeters, or one-eighth of an inch, in thickness.

[1] Determination lost by accident.

"2. The white, starchy-appearing part occupying the crown end of the kernel and usually also immediately surrounding, or partially surrounding, the germ.

"3. The germ itself which occupies the central part of the kernel toward the tip end.

"These different parts of the corn kernel can be readily recognized by merely dissecting a single kernel with a pocket-knife, and it may be added that this is the only instrument needed by anybody in making a chemical selection of seed corn by mechanical examination.

"The horny layer, which usually constitutes about sixty-five per cent. of the corn kernel, contains a large proportion of the total protein in the kernel.

"The white, starchy part constitutes about twenty per cent. of the whole kernel, and contains a small proportion of the total protein. The germ constitutes only about ten per cent. of the corn kernel, but while it is rich in protein, it also contains more than eighty-five per cent. of the total oil content of the whole kernel, the remainder of the oil being distributed in all the other parts.

"By keeping in mind that the horny layer is large in proportion, and also quite rich in protein, and that the germ, although rather small in proportion, is very rich in protein, so that these two parts contain a very large proportion of the total protein in the corn kernel, it will readily be seen that by selecting ears whose kernels contain more than the average proportion of germ and horny layer, we are really selecting ears which are above the average in their protein content. As a matter of fact, the method is even more simple than this, because the white, starchy part is approximately the complement of, and varies inversely as, the sum of the other constituents; and to pick out seed corn of high protein content it is only necessary to select those ears whose kernels show a relatively small proportion of the white, starchy part surrounding the germ.

"As more than eighty-five per cent. of the oil in the kernel is contained in the germ, it follows that ears of corn are relatively high or low in their oil content according as their kernels have a larger or smaller proportion of germ.

"In selecting seed corn by chemical analysis, we remove from the individual ear two adjacent rows of kernels as a representative sample. This sample is ground and analyzed as completely as may

be necessary to enable us to decide whether the ear is suitable for seed for the particular kind of corn which it is desired to breed. Dry matter is always determined in order to reduce all other determinations to the strictly uniform and comparable water-free basis. If, for example, we desire to change only the protein content, then protein is determined. If we are breeding to change both the protein and the oil, then determinations of both of these constituents must be made."

Any careful farmer can make such examinations as these. The relative abundance of one or the other of the three areas in the kernel will indicate what ears should be chosen for seed. Professor Hopkins proposes a system of field trials in which one ear furnishes plants for one row, thereby allowing the operator to see and measure the individuality of each ear. By choosing ears that most nearly approach the ideal, and then by continued selection year by year, the desired result is to be secured and maintained.

It is impossible to overestimate the value of any concerted corn-breeding work of this general type. The grain alone of the corn crop is worth about one billion dollars annually. It is no doubt possible to increase this efficiency by more than one per cent.

An interesting cognate inquiry to this direct breeding work is the study of the commercial grades of grains. It is a most singular fact that the dealer's "grades" are of a very different kind from the farmer's "varieties." In the great markets, for example, corn is sold as "Yellow No. 1," "Yellow No. 2," "Yellow No. 3." Any yellow corn may be thrown into these grades. What constitutes a grade is essentially a judgment on the part of every dealer. It so happens that the grade tends to deteriorate as the grain reaches the seaboard, for the tendency of each dealer is to mix with the better grades just as much of an inferior grade as will allow the carload or cargo to pass the inspector's examination. The result is that the grain is likely to be condemned or criticised when it reaches Liverpool. Complaints having come to the Government, the United States Department of Agriculture has undertaken to determine how far the grades of grain can be reduced to indisputable instrumental measurement. This work is now in the hands of Mr. Scofield, in the Division of Botany. The result is likely to be a closer defining of what a grade is; and this point once determined, the producer will make an effort to grow such grain as will grade to No. 1, and thereby reach the extra

price. Eventually the efficiency points of the grower and the commercial grades of the dealer ought nearly or quite to coincide. There should come a time when corn is sold on its inherent merits, as, for example, on its starch content. This corn would not then be graded 1, 2 and 3 on its starch content, because that content would be assured in the entire product; but the Grade 1 would mean prime physical condition, and the lower grades inferior physical condition. Eventually something like varietal names may be attached to those kinds of corns that, for example, grade fifteen per cent. protein. The name would be a guarantee of the approximate content, as it now is in a commercial fertilizer.

Closely allied to the corn-breeding work of Illinois (which is carried on by the Experiment Station and also by a commercial firm organized for that purpose) is the wheat-breeding and flax-breeding work in Minnesota under the direction of Professor Hays. Mr. Hays' aim has been chiefly to increase productiveness. The following sketch is made from his notes:

"Here are three examples of increased efficiency produced at the Minnesota Experiment Station in co-operation with the U. S. Bureau of Plant Industry.

"Minn. No. 163 wheat was bred by selection from Fife parentage. During three years' comparison in field tests at University Farm, near Minneapolis, it averaged 2.7 bushels gain per acre, or eleven per cent., better than its parent variety, as shown by the following table:

Minn. No. 163	28.5	bushels.
Fife parent	25.8	"
Increase	2.7	"

"In 1899, this wheat was sold to one hundred farmers, thirty-eight of whom made the comparison between this and their common wheats in a manner fair to both. The following table shows the average increased yield to have been 1.4 bushels per acre, or eight per cent.:

Minn. No. 163, average yield	18.1	bushels.
Common wheats, average yield	16.7	"
	1.4	"

"Minn. No. 169 wheat was bred by selection from a Blue Stem

foundation. During the first four years that it was in our field tests it averaged 4.9 bushels more than the parent wheat, as displayed by the following table of average yields, showing an increase over its parent variety of more than twenty per cent.:

Minn. No. 169	28.5	bushels.
Minn. No. 51	23.6	"
Gain	4.9	"

"In 1902, this wheat was sent in four-bushel lots, at $1.50 per bushel, to three hundred and seventy-five farmers. Eighty-nine reports gave comparisons that were fair both to the new and old wheats, and there were obtained the following average yields, showing an increase over the common wheats of the entire State of eighteen per cent. If this increase could be applied to one-tenth of the area of the wheat crop in Minnesota, the increased yield would be worth over a million dollars:

Minn. No. 169	21.5	bushels.
Common wheats	18.2	"
Increase	3.3	"

"The third example is even more pronounced. Seven years ago Prof. Hays chose seven samples of the common Minnesota and Dakota flax, and made by selection many new types for the production of seed, and numerous other types especially for production of fibre. The following table gives the general results:

	Yield of grain.	*Yield of straw.*	*Height in inches.*
Av. of 4 best varieties selected for seed	17.8	1.40	23
Av. of 4 best varieties selected for fibre	10.5	1.76	35
Av. of 4 best common varieties (from outside sources)	11.9	1.52	24
	5.9	.24	....

"Here in field trials, in 1902, the increased yield per acre of the new varieties bred for seed is forty-nine per cent.; and the increased height of the new varieties bred for fibre is forty-six per cent. more than the common flax."

"We have developed statistical methods," Professor Hays writes, "of dealing with such plants as wheat, alfalfa, corn, and,

in fact, nearly all of the field crops where it is necessary or very advantageous to grow or plant in a hill, that selections may be made and the breeding powers of parent plants measured. The general features of this statistical work may be stated as follows: Every acquisition or newly-bred variety receives a number written thus, 'Minn. No. 13 corn,' for example. It is also botanically described and the facts concerning its history, name, description, etc., entered in our *Minnesota Number Book*. If the newly-secured variety is an exceptionally promising one it is put into field tests, but ordinarily in the preliminary garden test the first year. Promising acquisitions and promising newly-bred hybrid stocks are entered in the nursery, where their breeding by rigid selection is begun, and large numbers of plants are grown, one in each hill, giving each plant the same space and opportunities as each other plant. By processes of elimination, the few best performers are secured. The next year we plant a large number of the progeny of each of these superior mother-plants. The average yield, height and other measures are taken of the progeny of each mother-plant. These tests of the breeding values of the mother-plants are continued two and sometimes three years. Seeds from parent plants producing the best average progeny are used alone or in mixtures of close-pollinated species, and in mixtures in open pollinated species as the foundation of new varieties. These are tested in the field with the parent and other best standard varieties for three years. Any introduced or newly-bred variety which is an especially good yielder of value per acre is sent to the co-operating State Experiment Stations in surrounding States and to our sub-stations, and its quantity is rapidly increased. Any variety that is specially promising after being tried for, say, two years at several stations is increased to sufficient quantity to sell to a number of farmers in each county in the State. This seed, backed by all the force of pedigree that we can command, is sold at a high price, so as to make the seed business profitable, and men are induced to raise it and sell large quantities at a price which will yield them a profit. In this way our first new wheat will be planted on hundreds of thousands of acres this year, and other new things are being widely disseminated."

A most gratifying augury of this coming type of effort is to be found in the work of the Plant-Breeding Laboratory of the national Department of Agriculture. This is an organization effected for

the purpose of producing types or kinds of plants that shall meet particular requirements. Its work is now proceeding with many groups of plants, but the burden of all its effort is efficiency in the final product. Its work with cotton promises to do nothing less than to revolutionize the cotton industry. The special difficulty with the present Upland cotton is the shortness of the "staple" or fibre. This inch-long staple sells at present (1903) for eight to eight and one-quarter cents a pound, whereas the long staple of the Sea Island cotton sells for twenty-five to thirty cents per pound. The effort is to secure a longer staple for the Upland, either by crossing it with the Sea Island or by working with some foreign long-staple type. The Egyptian cotton has a long staple, and this is now being used as one of the foundation stocks. But the Egyptian cotton possesses faults along with its long staple. It will be the work of years' by means of careful selection, to augment or maintain the desirable qualities and to eliminate the undesirable qualities; when this is done, the cotton will no longer be the Egyptian, but practically a new creation, and this new creation should receive a new name in order to distinguish it from the inferior Egyptian from which it will have had its birth. Under the leadership of Mr. Webber, this new plant-bleeding enterprise (probably the largest in the world) is now extended to citrous fruits, apples, pineapples, oats, tobaccos and other crops; and there is every indication that its usefulness will expand greatly within the immediate future. Other institutions, and other divisions of the Department of Agriculture, are conducting similar work. Time is now on when every resourceful farmer must look to the improving of the intrinsic merits of his crops.

The modern methods of plant-bleeding demand, first, that the breeder shall familiarize himself thoroughly with the characteristics of the group of plants with which he is to work. He must have very specific and definite knowledge of what makes the plant valuable and what its shortcomings are. Then he must secure as starting-points plants that give promise in the desired direction. Thereafter his skill will be taxed in selecting along responsive lines, making accurate and significant statistical measures, in devising workable systems of testing. He must grow large numbers of plants, if he is working with farm crops, in order to multiply his chances of securing desirable variations and to minimize the errors.

A promising course of breeding is one that shall develop disease-

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resisting races within the variety. Considerable progress has already been made in this direction with cotton, oats and some other crops. Now and then a hill or a row or a variety of potato resists the blight. Why? May it not be used as a starting-point for the development of a blight-resistant strain? The development of disease-resisting and pest-resisting races is one of the most promising developments in the new plant pathology.

Nor are all these advances to be secured from seed selection alone. The cuttings and grafts of fruit plants perpetuate the parental characteristics with a good degree of surety. The time must soon come when it will not be sufficient to multiply the Bartlett pear from the Bartlett pear. We shall still further specialize our ideals and propagate from particular Bartlett pear trees that have made record performances. This subject is being tested in New York and elsewhere. It is one of the most important problems now before the nurseryman and orchardist.

All this plant breeding work is especially of a kind to demand governmental support. The progress of invention can be left to private initiative, because the person can patent his device and secure all the financial returns that it is worth. A variety cannot well be patented or controlled. This is particularly true of these great race improvements, in which no distinct and namable variety results; and these race improvements are the very ones that are most likely to be of greatest benefit to agriculture and therefore to the nation.

These methods and ideals may all be summed up as follows:

I. To determine on what the merit in any group of plants depends, and to find out what is needed to make the plants more efficient. What makes a potato "mealy"?

II. Securing a start in the desired direction by

(*a*) Choosing for seed-bearing any plants that are promising;

(*b*) Introducing prominent foundation-stock from other regions or other countries;

(*c*) Crossing for the purpose of injecting a new or better character into the strain.

III. Continued selection, careful testing and accurate statistical measurements and records to keep the progress true to line.

The first thing that strikes one in all this new work is its strong contrast with the old ideals. The "points" of the plants are those of "performance" and "efficiency." It brings into sharp relief

the accustomed ideas as to what are the "good points" in any plant, illustrating the fact that these points are for the most part only fanciful, are founded on *a priori* judgments, and are more often correlated with mere "looks" than with efficiency. An excellent example may be taken from corn. In "scaling" any variety of corn, it is customary to assume that the perfect ear is one nearly or quite uniformly cylindrical throughout its length and having the tip and butt well covered with kernels. In fact, the old idea of a good variety of corn is one that bears such ears. Now this ideal is clearly one of perfection and completeness of mere form. We have no knowledge that such form has any correlation with productiveness, hardiness, drought-resisting qualities, protein or starch content—and yet these attributes are the ones that make corn worth growing at all. An illustration also may be taken from string beans. The ideal pod is considered to be one of which the tip-projection is very short and only slightly curved. This apparently is a question of comeliness, although a short tip may be associated in the popular mind with the absence of "string" in the pod; but we do not know that this character has any relation to the efficiency of the bean pod. We are now undergoing much the same challenging of ideas respecting the "points" of animals. These "points," by means of which the animals are "scored," are in large part merely arbitrary. Now, animals and plants are bred to the ideals expressed in these arbitrary points, by choosing for parents the individuals that score the highest. When it becomes necessary to recast our "scales of points," the whole course of evolution of domestic plants and animals is likely to be changed.

We are to breed not so much for merely new and striking characters that will enable us to name, describe and sell a "novelty," as to improve the performance along accustomed lines. We do not need new varieties of seedling potatoes so much as we need to improve, by means of selection, some of the varieties that we already possess. We are not to start with a variety, but with a plant. It is possible to secure a five per cent. increase in the efficiency of our field crops; this would mean the annual addition of hundreds of millions of dollars to the national gain.

The purpose, then, of our new plant-breeding is to produce plants that are more efficient for specific uses and specific regions. They are to be specially adapted. These efficiency-ideals are of six general categories:

1. Yield ideals.
2. Quality ideals.
3. Seasonal ideals.
4. Physical conformation ideals.
5. Regional adaptation ideals—as to climate, altitude, soil.
6. Resistant ideals—as to diseases and insects.

The main improvement and evolution of agriculture are going to come as the result of greater and better crop yield and greater and better animal production. It is not to come primarily from invention, good roads, rural telephone, legislation, discussion of economics. All these are merely aids. Increased crop and animal production are to come from two agencies: improvement in the care that they receive; improvement in the plants and animals themselves. In other words, the new agriculture is to be built upon the combined results of better cultivation and better breeding. So far as the new breeding is concerned, it is characterized by perfect definiteness of purpose and effort, the stripping away of all arbitrary and factitious standards, the absence of speculative theory and the insistence upon the great fact that every plant and animal has individuality.

CORNELL UNIVERSITY, ITHACA, N. Y.

THE CURTIS STEAM TURBINE.

BY W. L. R. EMMET.

(*Read April 2, 1903.*)

The development which this paper describes is based upon the original theories and inventions of Mr. C. G. Curtis, of New York, whose ideas were first made the subject of patent application about 1895. Since that time these inventions have been the subject of experimental investigation at Schenectady, under the direction of Mr. Curtis and of the General Electric Company's engineers; the object of these experiments being to establish data and laws which would form a basis for the correct design of commercial apparatus. The difficulties of such an investigation are very great. All new facts must be established by the tests of different machines or parts which are difficult and expensive to produce. About two years ago

the results of these experiments gave us data which showed great commercial possibilities, and since that time work has gone on on a large scale in the production of commercial machines. The contracts for these machines now aggregate 230,000 H. P. in turbine-driven electric generating units, the largest size so far built being 7500 H. P. Thus a great industry has been brought into existence in a very short time, and since the work has all been done in one place and by a few persons very little information concerning it has reached the public. This paper is the first printed matter which has appeared on the subject.

The reason for this immense demand and production, without publicity and in so short a time, is that the improvements effected are radical in economy, simplicity and efficiency of action.

All improvements in prime movers are of great importance to the engineering world. The steam turbine is destined to effect the first really great improvement since the days of Watt, and the forms of Curtis turbine here described make the first great stride in advance of other steam engines.

Every efficient steam engine must provide means by which a fair proportion of the expansive force of steam can be converted into useful work. In the engines of James Watt and his successors this result is accomplished in various degrees by the application of pressure from the steam to moving pistons. In steam turbines the expansive force imparts motion to the steam itself, and this motion is given up to a revolving part by impacts of the moving steam upon it.

The idea of the steam turbine is quite simple, and is similar to that of the water turbine or impulse wheel. The practical difficulty which has heretofore prevented the development of good steam turbines lies in the very high velocity which steam can impart to itself in expansion, and the difficulty in efficiently transferring this motion to wheels at speeds practicable for construction or practical use. Steam expanding from 150 pounds gauge pressure per square inch into the atmosphere is capable of imparting to itself a speed of 2950 feet per second, and if it is expanded from 150 pounds gauge pressure into a 28-inch vacuum it can attain a velocity of 4010 feet per second. The spouting velocity of water discharged from a nozzle with 100 feet head is 80 feet per second. These figures illustrate the very radical difference of condition between water turbines and steam turbines. In both water and steam tur-

bines the theoretical condition of maximum economy exists when the jet of fluid moves with a velocity equal to about twice that of the vane against which it acts. In water-wheels this relation is easily established under all conditions, while with steam the total power produces a velocity so high that the materials available for simple wheels and vanes are not capable of sustaining a proper speed relation to it under practicable conditions.

Before the appearance of the Curtis turbine two practical methods of accomplishing fair economy had been devised, namely, the turbines of Carl De Laval, of Sweden, and of Hon. Charles Algernon Parsons, of England, both of which were brought out more than fifteen years ago. FROM 1902

In the De Laval turbine the total power of the steam is devoted to the production of velocity in an expanding nozzle, which produces velocity very efficiently. The jet so produced is delivered against a set of vanes on a single wheel which, by an ingenious construction and method of suspension, is adapted to operation at a very high peripheral velocity. The very high rotative speed which this construction entails is made available for dynamo driving by very perfectly made spiral-cut gears which effect a ten-to-one speed reduction. The peripheral velocity of the wheel in the largest De Laval turbines is about 1200 feet per second, while the velocity which energy can impart to steam is over 4000 feet per second. Thus the wheel falls far short of the theoretically economical speed.

In the Parsons turbine the steam is carried in an axial direction through the space provided, between a succession of internal revolving cylinders and external stationary cylinders which enclose them. Both the internal and the external cylindrical surfaces are covered by many successive circles of vanes so arranged that the steam has to pass alternately through rows of moving and stationary vanes. In passing through this turbine the steam never acquires a speed which approaches the velocity which it attains in the De Laval nozzle; but instead moves along alternately, acquiring velocity by expansion, and partially giving it up by impact with the moving vanes.

Both of these turbines have attained some success, but neither, as thus far developed, affords sufficient advantage over the steam engine to cause any very rapid or radical change in engineering conditions.

The important disadvantages of the De Laval type are, that it is

limited by the imperfections of high-speed gearing, that its efficiency is not particularly high, and that the design is not conveniently applicable to large sizes. The Parsons type is principally limited by the multiplicity and weight of its parts, and the high cost of construction.

The Curtis turbine retains some of the features of its predecessors, but introduces new ideas which make possible a much lower speed, less weight, fewer and simpler parts, higher economy, less cost, and other important advantages.

The general arrangement of a turbine generating-unit of this type is shown by the drawings which accompany this paper. Its functions may be briefly described as follows, and are illustrated by the accompanying cut:

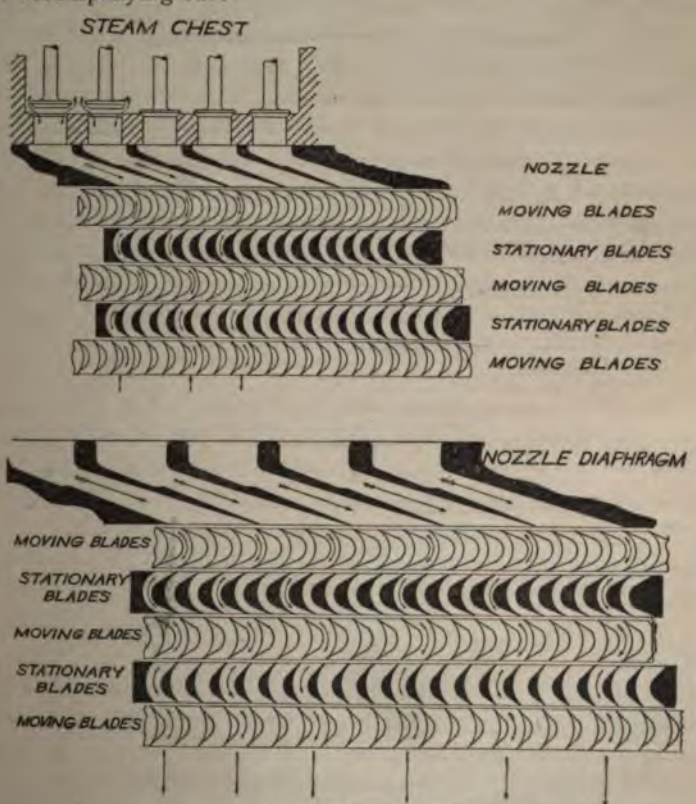


Diagram of Nozzles and Buckets in Curtis Steam Turbine.

Velocity is imparted to the steam in an expanding nozzle so designed as to efficiently convert nearly all the expansive force, between the pressure limits used, into velocity in the steam itself. After leaving the nozzle, the steam passes successively through two or more lines of vanes on the moving element, which are placed alternately with reversed vanes on the stationary element. In passing successively through these moving and stationary elements, the velocity acquired in the nozzle is fractionally abstracted, and largely given up to the moving element. Thus the steam is first thrown against the first set of vanes of the moving element, and then rebounds alternately from moving to stationary vanes until it is brought nearly to rest. By this means a high steam velocity is made to efficiently impart motion to a comparatively slowly moving element. The nozzle is generally made up of many sections adjacent to each other, so that the steam passes to the wheels in a broad belt when all nozzle sections are in flow.

This process of expansion in nozzle and subsequent abstraction of velocity by successive impacts with wheel vanes is generally repeated two or more times, the devices for each repetition being generally designated as a stage. There may be various numbers of stages and various numbers of lines of moving vanes in each stage. The number of stages and the number of lines of vanes in a stage are governed by the degree of expansion, the peripheral velocity which is desirable or practicable, and by various conditions of mechanical expediency.

Generally speaking, lower peripheral speeds entail more stages, more lines of vanes per stage, or both. Our general practice is to so divide up the steam expansion, that all stages handle about equal parts of the total power of the steam.

The losses and leakages of the earlier stages take the form of more heat or more steam for the later stages, and are thus in part regained. Much water of expansion, which might occasion loss by re-evaporation, is drained out of each stage into that which succeeds it.

The governing is effected by successive closing of nozzles and consequent narrowing of the active steam belt. The cut shows part of the nozzle open and part closed; the arrows showing space filled by live steam. In the process of governing, the nozzles of the later stages may or may not be opened and closed so as to maintain an adjustment proportional to that of the first stage, which is

always the primary source of governing. Some improvement of light-load economy may be effected by maintaining a relative adjustment of all nozzles; but in many cases the practical difference in economy is not great, and automatic adjustment of nozzle opening in later stages is dispensed with in the interest of simplicity. In some machines an approximate adjustment is maintained by valves in later stages, which open additional nozzles in response to increases of pressure behind them. These are used as much for limiting the pressures in stage chambers as for maintaining the light load economy.

The principle of the Curtis steam turbine is susceptible of application to a variety of purposes Within the scope of this paper I intend to give only a general idea concerning existing designs for its application to electric generators. Its development, even for this purpose, is very recent, and will doubtless be subject to important future improvements. In its present state, however, it embodies many important advantages, as has already been stated. The most important of these advantages is the high steam economy which it affords under average conditions of service. This economy is shown by the accompanying curves, which are derived from actual tests of the first commercial machine of this type which was completed. This machine drives a dynamo of 600 Kw. capacity. The curves give its performance at a speed of 1500 R.P.M., which is a safe and practical speed for commercial operation, and which corresponds to a peripheral velocity of about 420 feet per second. The results, with superheat, given in these curves are not derived actually from tests of this turbine, but are plotted from data obtained on smaller turbines. They correspond to the results obtained on turbines of other types and are undoubtedly reliable.

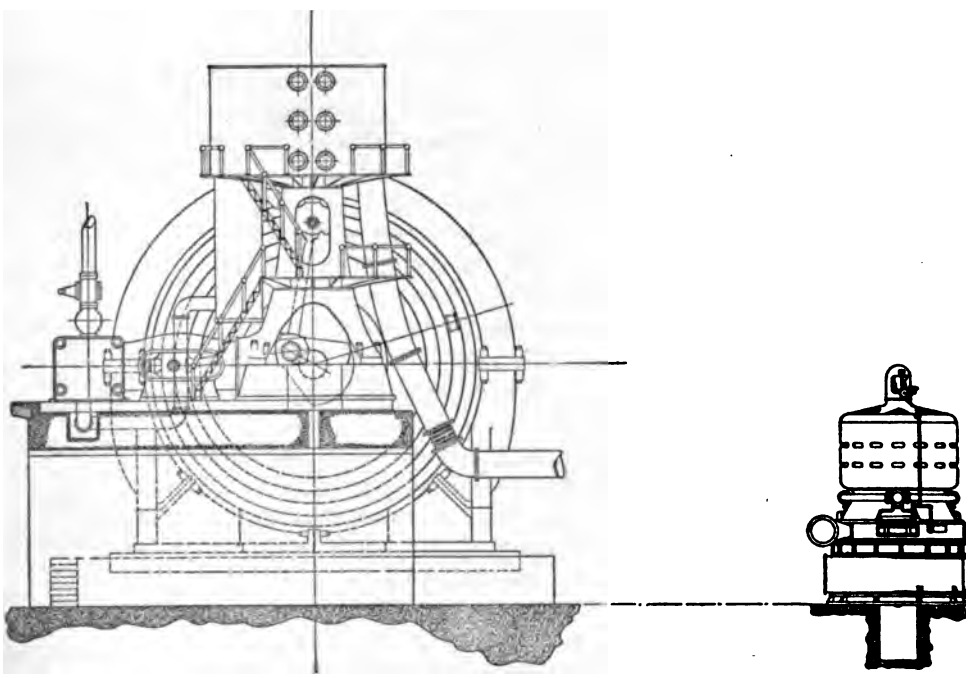
Curve 1 shows the steam consumption of this machine in pounds per kilowatt-hour output at various loads and under the conditions stated, the lower curve giving the steam consumption at various loads with 150 degrees superheat.

Curve 2 shows the results which could be obtained from this turbine if it were operated with high pressure and a high degree of superheat, these conditions of operation being perfectly practical with the machine, while with steam engines the use of such high temperatures would with ordinary constructions be prohibitive.

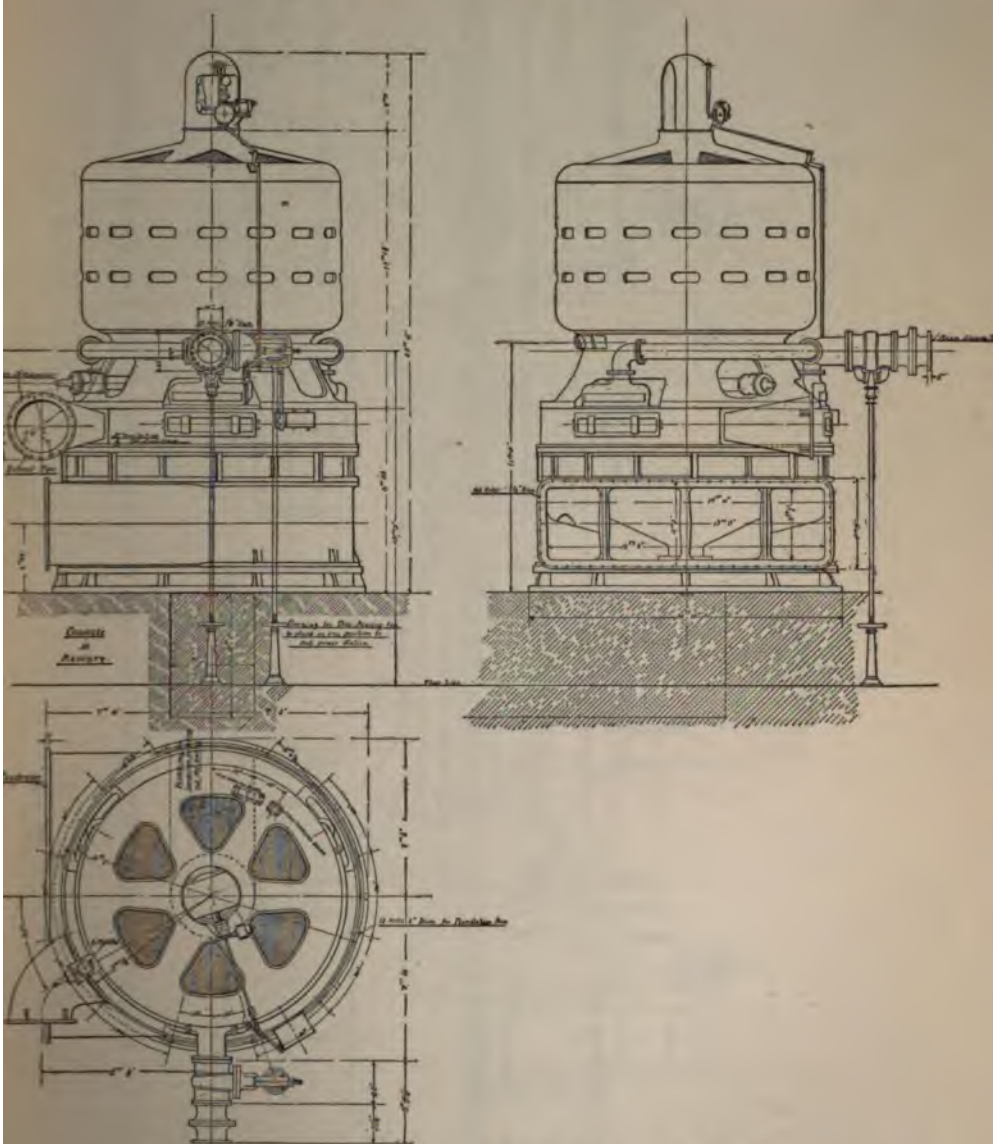
The results shown by these curves are better than any heretofore produced by steam turbines of any make or size, and are very much

by Corliss, a certain improvement in engine economy was effected, and although this improvement was accompanied by no diminution in cost, the change resulted in a very great activity in engine building, and the renewal of most of the large mill engines in the country. It is, therefore, safe to predict that the influence of the steam turbine will be of radical importance. The steam turbine is, on account of its high speed, particularly adapted to the driving of electric generators, and its introduction will consequently stimulate the use of electricity rather than other power transmitters.

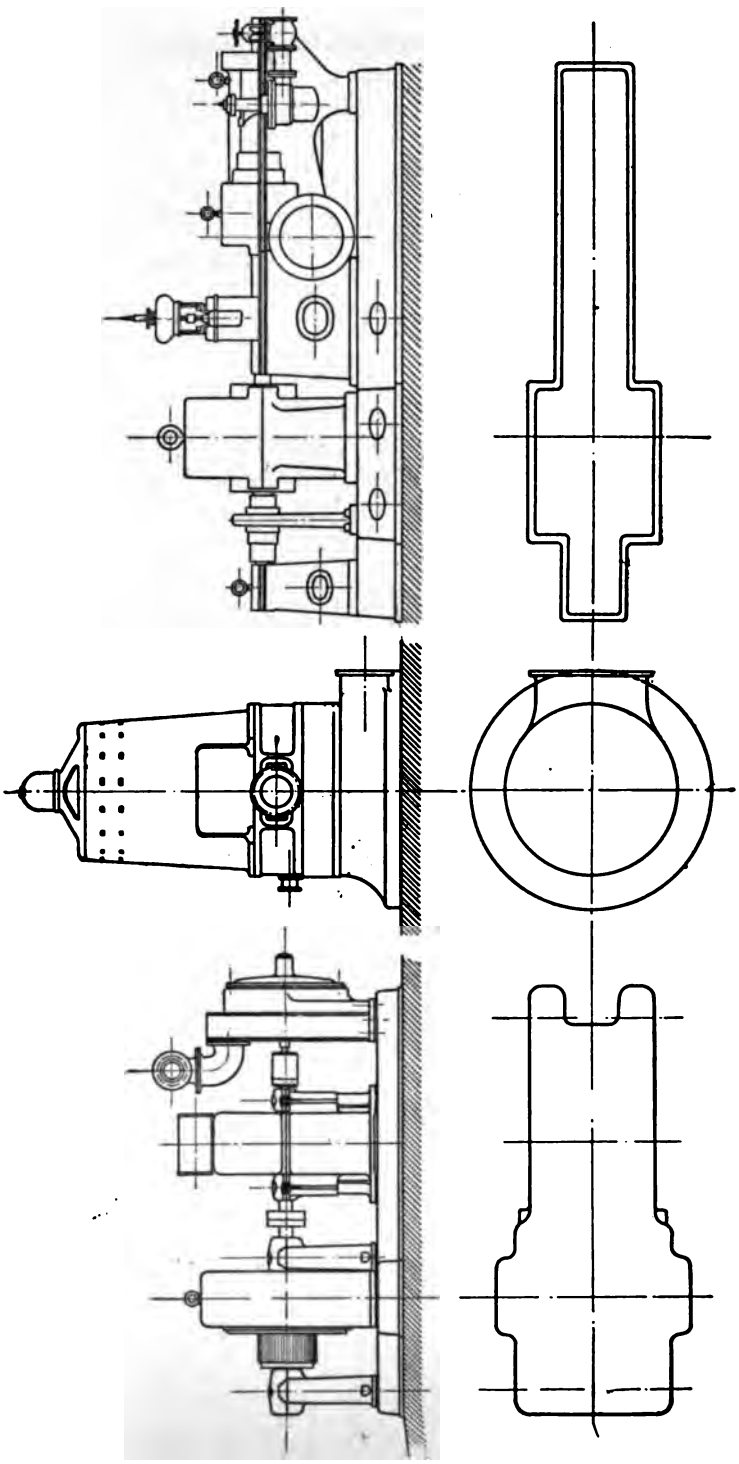
In the past the most economical use of steam has been confined to the most expensive and elaborate plants, while in the future it will be within the reach of all where condensing water is available.



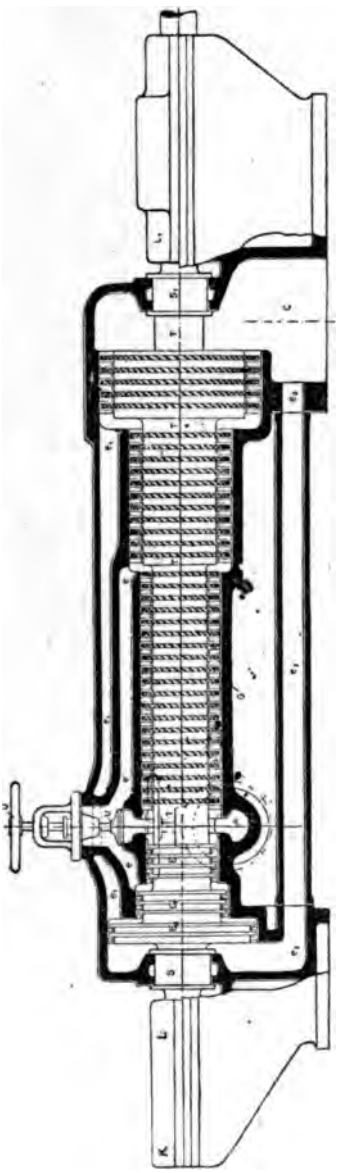
Comparative sizes of 5000 Kw., 75 R.P.M. Corliss Engine and 5000 Kw., 500 R.P.M. Curtis Turbine



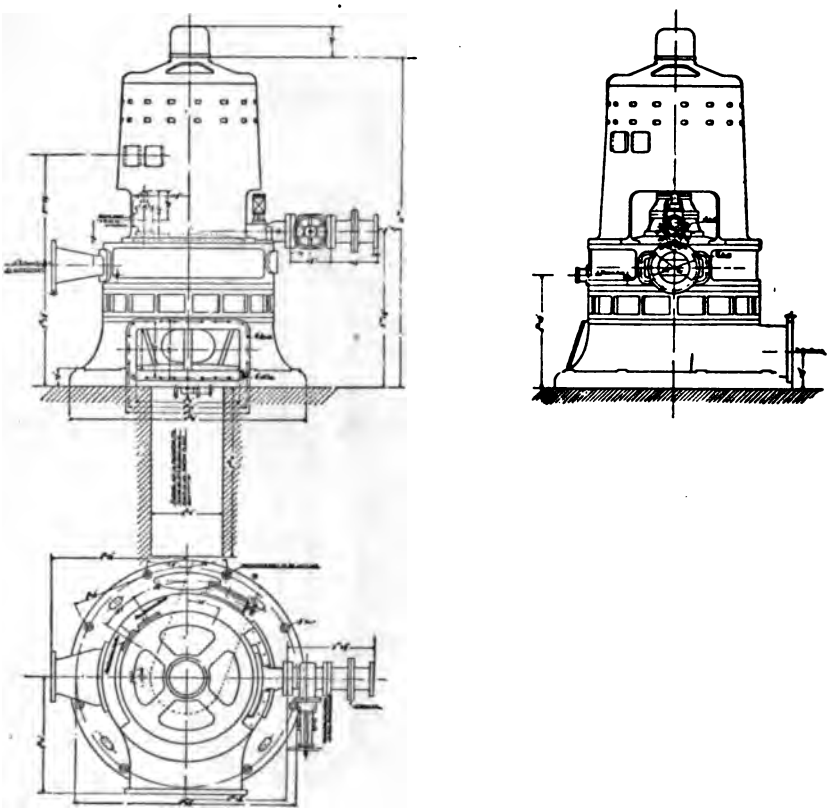
Plan and Elevation of 5000 Kw., 500 R.P.M. Curtis Turbine with Generator.



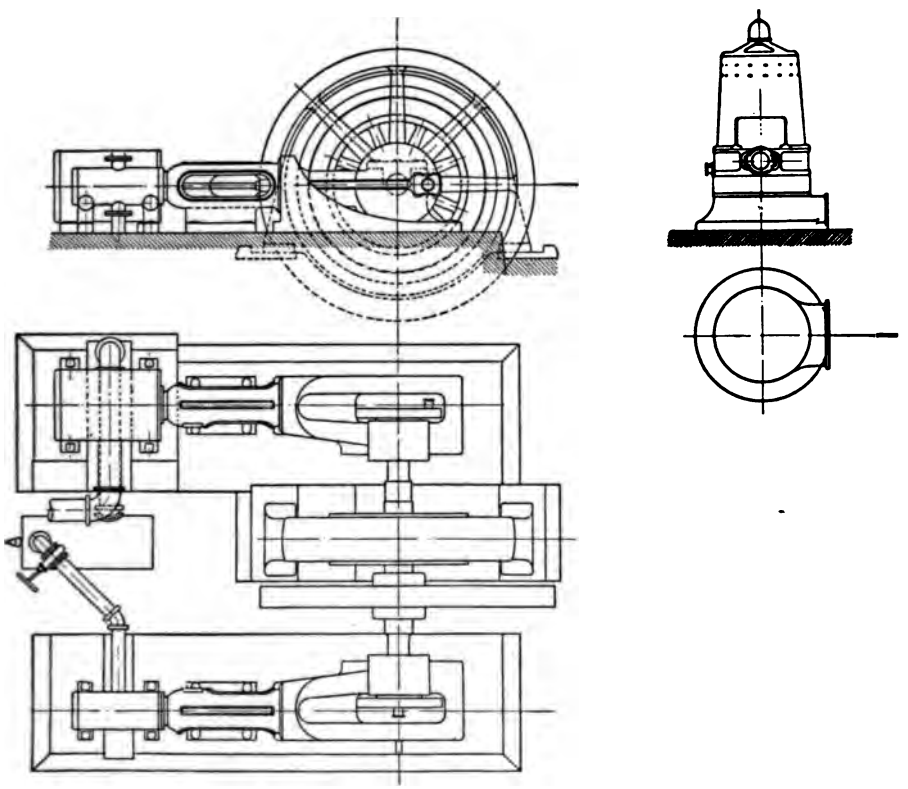
Comparison of 200 Kw., 9000 R.P.M. De Laval Turbine, 500 Kw., 1800 R.P.M. Curtis Turbine and 375 Kw., 3600 R.P.M. Parsons Turbine.



Cross-section of Parsons Turbine without Generator.

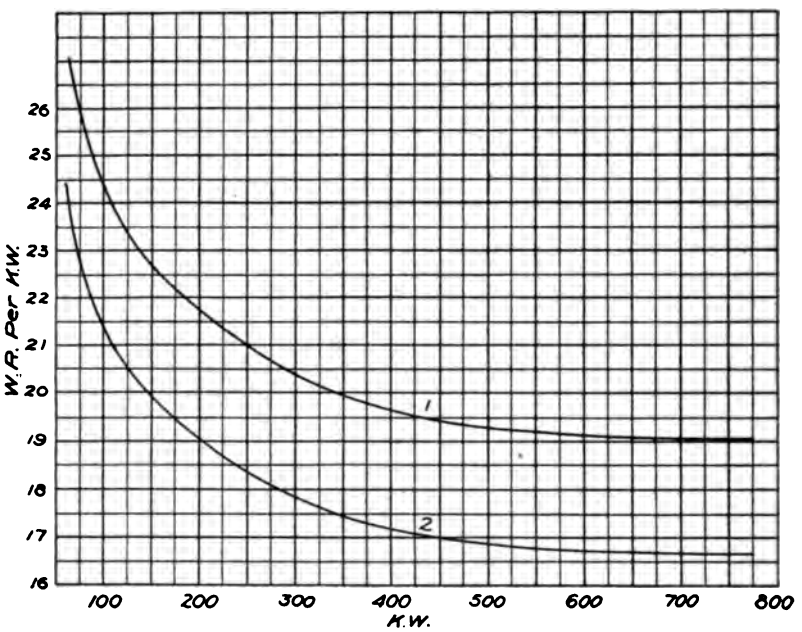


Plan and Elevation of 500 Kw., 1800 R.P.M. Curtis Turbine with Generator.



Comparison of 500 Kw., 100 R.P.M. Cross compound Engine and 500 Kw., 1800 R.P.M. Curtis Turbine.

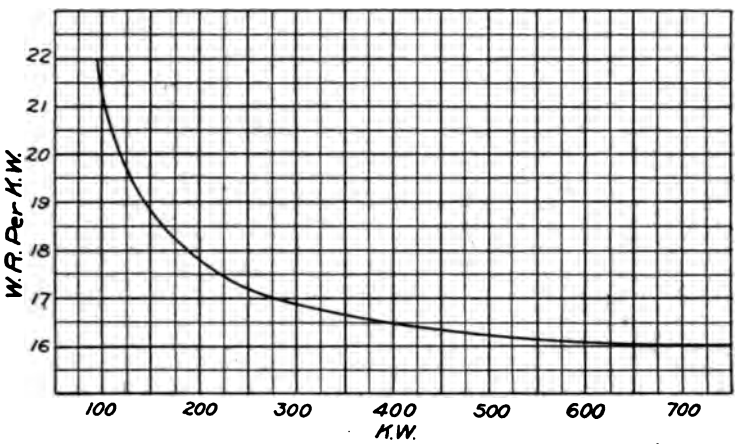
PROC. AMER. PHILOS. SOC. XLII. 172. F. PRINTED MAY 27, 1903.



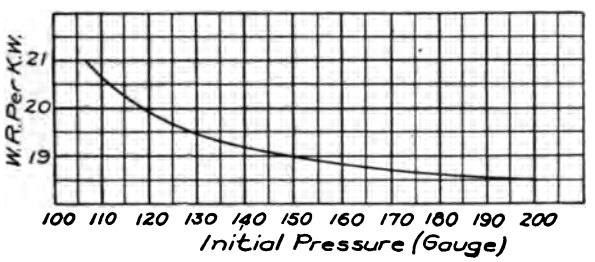
CURVE 1.—Curve showing water consumption, in pounds per Kw. hour, of 600 Kw. Curtis Steam Turbine, operating at 1500 R.P.M., with 140 lbs. gauge pressure and 28.5″ of vacuum.

1 Without superheat.

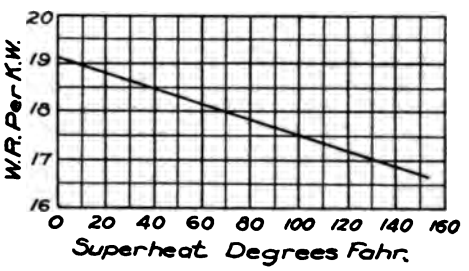
2 With 150° F. superheat.



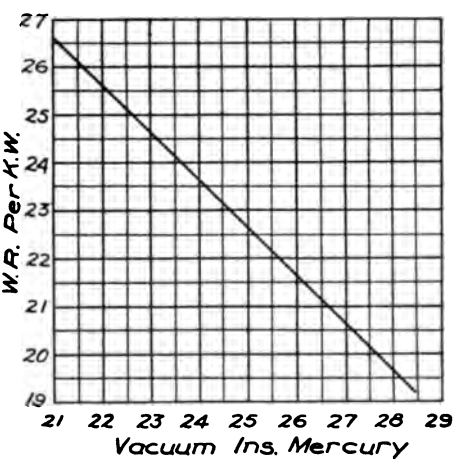
CURVE 2.—Curve showing water consumption, in pounds per Kw. hour, of 600 Kw. Curtis Steam Turbine, with different loads; speed 1500 R.P.M.; vacuum 28.5″; pressure 200 lbs. gauge, with 150° F. superheat.



CURVE 3.—Curve showing water consumption, in pounds per Kw. hour, of 600 Kw. Curtis Steam Turbine, at full load with different initial pressures; speed 1500 R.P.M.; vacuum 28.5″.



CURVE 4.—Curve showing water consumption, in pounds per Kw. hour, of 600 Kw. Curtis Steam Turbine, with different degrees of superheat when operating with full load at 1500 R.P.M.; vacuum 28.5″; pressure 140 lbs. gauge.



CURVE 5.—Curve showing water consumption, in pounds per Kw. hour, of 600 Kw. Curtis Steam Turbine, at full load with different degrees of vacuum; speed 1500 R.P.M; steam pressure 140 lbs. gauge.

SCHENECTADY, N. Y., APRIL 2, 1903.

NEW APPLICATIONS OF MACLAURIN'S SERIES IN THE SOLUTION OF EQUATIONS AND IN THE EXPANSION OF FUNCTIONS.

BY P. A. LAMBERT.

(*Read April 3, 1903.*)

I.—INTRODUCTION.

The modern theory of differential equations is based on the expansion by Maclaurin's series of the solutions of the equations in infinite series. The striking analogy existing between the theory of algebraic equations and the theory of differential equations suggested the possibility of expressing the solutions of algebraic equations in series to be obtained by an application of Maclaurin's series. After some experimenting the author happened on the device of introducing a factor x into all the terms but two of the equation $f(y)=0$, whereby y becomes an implicit function of x. The successive x-derivatives of y are now formed, and together with y are evaluated for $x=0$. By Maclaurin's series the expansions of y in powers of x become known. If x be made unity in these expansions, the roots of $f(y)=0$ are found, provided the resulting series are convergent.

To illustrate this method, consider the equation

$$(1) \quad y^4 - 3y^2 + 75y - 10000 = 0.$$

Maclaurin's series

$$y = y_0 + \frac{dy_0}{dx_0}x + \frac{d^2y_0}{dx_0^2}\frac{x^2}{2!} + \frac{d^3y_0}{dx_0^3}\frac{x^3}{3!} + \frac{d^4y_0}{dx_0^4}\frac{x^4}{4!} + \cdots,$$

where $y_0, \frac{dy_0}{dx_0}, \frac{d^2y_0}{dx_0^2}, \frac{dy^3_0}{dx_0^3}, \frac{d^4y_0}{dx_0^4}, \ldots$ stand for the values of $y, \frac{dy}{dx}, \frac{d^2y}{dx^2}, \frac{d^3y}{dx^3}, \frac{d^4y}{dx^4}, \ldots$ when x is made zero, expands y, a function of x, in powers of x.

By introducing a factor x in the second and third terms of (1) an equation is formed

$$(2) \quad y^4 - 3xy^2 + 75xy - 10000 = 0$$

which defines y as an implicit function of x.

Differentiating (2) twice in succession

$$(3)\quad 4y^3\frac{dy}{dx}-3y^2+75y-6xy\frac{dy}{dx}+75x\frac{dy}{dx}=0$$

$$(4)\quad 4y^3\frac{d^2y}{dy^2}+12y^2\left(\frac{dy}{dx}\right)^2-12y\frac{dy}{dx}+150\frac{dy}{dx}-6x\left(\frac{dy}{dx}\right)^2-$$

$$6xy\frac{d^2y}{dx^2}+75x\frac{d^2y}{dx^2}=0.$$

Making x zero in (2), (3) and (4)

$$y_0=+\ 10,-10,\quad +10\sqrt{-1}.\qquad -10\sqrt{-1}$$

$$\frac{dy_0}{dx_0}=-.1125,-.2625,+.\ 1875-.075\sqrt{-1},+.1875+\ .075\sqrt{-1}$$

$$\frac{d^2y_0}{dx_0^2}=-.0029,-.0029,-.0000015+.0039\sqrt{-1},-\ .0000015-.0039\sqrt{-1}.$$

Substituting these four sets of values in Maclaurin's series and placing $x=1$, the roots of equation (1) are found to be

$$y_1=+9.886,\ y_2=-10.261,\ y_3=+.1875+9.937\sqrt{-1},$$

$$y_4=+.1875-9.927\sqrt{-1},$$

all correct to the last decimal.

This method will be applied to the solution (II) of trinomial algebraic equations, (III) of general algebraic equations, (IV) of trinomial transcendental equations, and finally (V) the method will be applied to obtain expansions commonly obtained by Lagrange's series.

II.—Trinomial Algebraic Equations.

The general trinomial equation of degree n has the form

$$(1)\quad y^n-nay^{n-k}-b=0.$$

Introducing a factor x in the second term of (1)

$$(2)\quad y^n-naxy^{n-k}-b=0.$$

Applying the method and denoting the n^{th} root of b by ω

$$(3)\quad y=\omega+\omega^{1-k}a+\omega^{1-2k}(1-2k+n)\frac{a^2}{2!}$$

$$+\omega^{1-3k}(1-3k+n)(1-3k+2n)\frac{a^3}{3!}$$

$$+\omega^{1-4k}(1-4k+n)(1-4k+2n)(1-4k+3n)\frac{a^4}{4!}+\ldots.$$

To determine when series (3) is convergent, group the terms numbered $1, n+1, 2n+1, 3n+1, \ldots$, then those numbered $2, n+2, 2n+2, 3n+2, \ldots$, finally those numbered $n, 2n, 3n, 4n, \ldots$. Each of these n partial series is found by Cauchy's ratio test to be convergent when a^n is numerically less than $k^{-k}(n-k)^{k-n}b^k$. When this condition of convergency is satisfied series (3), by substituting for ω in succession each of the n values of the n^{th} root of b, determines the n roots of equation (1).

By introducing the factor x in the third term of equation (1) and applying the method a series is obtained which determines k roots of equation (1), and by introducing the factor x in the first term of equation (1) a series is obtained which determines $n-k$ roots of equation (1). The two series thus obtained are convergent when a^n is numerically greater than $k^{-k}(n-k)^{k-n}b^k$. When $a^n=k^{-k}(n-k)^{k-n}b^k$ equation (1) has equal roots. There is therefore developed a complete theory of trinomial equations.

The general fifth degree equation can, by Tschirnhausen transformations requiring the solution of equations of the second and third degrees only, be transformed into the trinomial equation $y^5+ay+b=0$. If a^5 is numerically less than $\frac{625}{256}b^4$, the five roots of this equation are found by applying the method to $y^5+axy+b=0$. If a^5 is numerically greater than $\frac{625}{256}b^4$, the five roots are found by applying the method to $y^5+ay+bx=0$ and $xy^5+ay+b=0$. If a^5 numerically equals $\frac{625}{256}b^4$, the fifth degree equation has equal roots, and the removal of the equal roots makes the solution of the fifth degree equation depend on the solution of an equation of a degree not higher than the third. A third degree equation becomes trinomial by removing the second term, which is accomplished by a linear transformation. The method of this paper therefore effects the complete solution of the general fifth degree equation in infinite series.

In Weber's Algebra, volume I, pages 396-399, the real and imaginary roots of the equation $x^3-2x-2=0$ are computed by a method invented by Gauss for the solution of trinomial

equations. The convergency test shows that the series found by introducing the variable factor in the second term is convergent. Now the mathematician is satisfied when the convergency of the infinite series he uses is established, but the computer desires that the infinite series he is obliged to use shall converge rapidly. By transforming the equation $x^3 - 2x - 2 = 0$ into another lacking the first power, which is accomplished by placing $x = \frac{3}{3y-1}$, the equation $54y^3 - 18y - 23 = 0$ is found. The series found by applying the method to $54y^3 - 18xy - 23 = 0$ converges much more rapidly than the series obtained from the original equation. Differentiating $54y^3 - 18xy - 23 = 0$ four times in succession and making x zero,

$$y_0 = .7524, -.3762 \pm .3762\sqrt{-3}$$

$$\frac{dy_0}{dx_0} = .1477, -.0738 \mp .0738\sqrt{-3}$$

$$\frac{1}{2!}\frac{d^2y_0}{dx_0^2} = 0, \quad 0$$

$$\frac{1}{3!}\frac{d^3y_0}{dx_0^3} = -.0019, +.0010 \mp .0010\sqrt{-3}$$

$$\frac{1}{4!}\frac{d^4y_0}{dx_0^4} = .0004, -.0002 \mp .0002\sqrt{-3}.$$

The three values of y are .889 and $-.4492 \pm .3012\sqrt{-3}$, the corresponding values of x are 1.768 and $-.8847 \mp .5898\sqrt{-1}$. If the computations are made by logarithms they are not very lengthy.

The equation $y^4 - 11727\,y + 40385 = 0$ occurs in a paper by Mr. G. H. Darwin "On the Precession of a Viscous Spheroid," published in the *Philosophical Transactions of the Royal Society*, Part II, 1879, page 508. The convergency test shows that the factor x must be introduced in the last and in the first terms. The equation therefore has two real positive and two imaginary roots. Applying the method to

$$y^4 - 11727y + 40385x = 0,$$

$$y_0 = \quad 22.720, \quad -11.360 \pm 11.360\sqrt{-3}$$

$$\frac{dy_0}{dx^0} = -\ 1.148, \quad -\ 1.148$$

$$\frac{1}{2!}\frac{d^2y_0}{dx_0^2} = -\ .116, \qquad .058 \pm \quad .058\sqrt{-3}$$

$$\frac{1}{3!}\frac{d^3y_0}{dx_0^3} = -\ .019, \qquad .010 \mp \quad .010\sqrt{-3}$$

$$\frac{1}{4!}\frac{d^4y_0}{dx_0^4} = -\ .004, \quad -\ .004$$

Three roots of the equation are 21.432 and $12.444 \pm 19.759\sqrt{-1}$. Applying the method to

$$xy^4 - 11727y + 40385 = 0,$$

$$y_0 = 3.4436, \frac{dy_0}{dx_0} = .0120, \tfrac{1}{2}\frac{d^2y_0}{dx_0} = .0002.$$

The fourth root of the equation is 3.4558.

This method applied to trinomial equations proves that an equation of degree n has n roots, determines how many roots are real, and presents a uniform scheme for computing all the roots, real and imaginary.

III.—General Algebraic Equations.

The method applied to the complete equation of degree n furnishes $\frac{n(n-1)}{1.2}$ series, and it becomes necessary to determine which of these series give n convergent series for the roots of the equation and if possible to insure rapidity of convergence of these n series.

Suppose the equation of degree n to be

$$\underline{ay^n} + a_0y^{n-1} + \ldots + a_k y^{n-k+1} + \underline{by^{n-k}} + b_0y^{n-k-1} + \ldots$$
$$+ b_1y^{n-k-l+1} + \underline{cy^{n-k-l}} + c_0y^{n-k-l-1} + \ldots + c_my^{n-k-l-m+1}$$
$$+ \underline{dy^{n-k-l-m}} + d_0y^{n-k-l-m-1} + \ldots + ry + \underline{s} = 0,$$

and suppose the terms which are underscored to be the terms from which the two terms into which the factor x is not introduced must be selected by taking consecutive terms in regular order from the left. The problem is how to recognize the terms which must be underscored.

If the factor x is omitted from the first two underscored terms $y_0=\left(-\frac{b}{a}\right)^{\frac{1}{k}}$; if from the second and third underscored terms $y_0=\left(-\frac{c}{b}\right)^{\frac{1}{l}}$; if from the third and fourth underscored terms $y_0=\left(-\frac{d}{c}\right)^{\frac{1}{m}}$; if from the last two underscored terms $y_0=\left(-\frac{s}{d}\right)^{\frac{1}{n-k-l-m}}$. Altogether n values of y_0 are found, and it is seen at a glance what values of y_0 are real and what are imaginary. In order that these values of y_0 shall be close approximations of the roots of the given equation, the successive derivatives $\frac{dy_0}{dx_0}$, $\frac{d^2y_0}{dx_0^2}$, $\frac{d^3y_0}{dx_0^3}$, $\frac{d^4y_0}{dx_0^4}$, ... must be small.

Forming $\frac{dy_0}{dx_0}$ corresponding to $y_0=\left(-\frac{b}{a}\right)^{\frac{1}{k}}$ and assuming that c is of such a magnitude that the term containing c overshadows all the other terms in the numerator of $\frac{dy_0}{dx_0}$, it is found that $\frac{dy_0}{dx_0}$ is necessarily small if the ratio of b^{k+1} to a^lc^k is numerically large. This same condition insures that the following derivatives $\frac{d^2y_0}{dx_0^2}$, $\frac{d^3y_0}{dx_0^3}$, ... are small.

In like manner it is shown that the derivatives corresponding to $y_0=\left(-\frac{c}{b}\right)^{\frac{1}{l}}$ are small provided the ratio of c^{l+m} to b^md^l is numerically large, and that the derivatives corresponding to $y_0=\left(-\frac{d}{c}\right)^{\frac{1}{m}}$ are small provided the ratio of d^{n-k-l} to $c^ms^{n-k-l-m}$ is numerically large. This ratio should, if possible, be made larger than 10 to insure rapid convergence.

The directions for underscoring terms are therefore as follows:

Underscore the first and last terms of the equation. Such other terms are to be underscored as satisfy the condition that if any three consecutive underscored terms be chosen, the ratio of the coefficient of the middle term with an ex-

ponent equal to the difference of the degrees of the first and third terms to the product of the coefficient of the first of the three terms with an exponent equal to the difference of the degrees of the second and third terms and the coefficient of the third term with an exponent equal to the difference of the degrees of the first and second terms shall be a large number.

To illustrate the method, the following equations are discussed:

$$(a)\quad \underline{y^5} - \underline{10y^3} + \underline{6y} + \underline{1} = 0.$$

Here all the terms are underscored, for the ratio of 10^4 to 6^2 is large, and the ratio of 6^1 to 10 is large. The method must be applied to (1) $y^5 - 10y^2 + 6xy + x = 0$, (2) $xy^5 - 10y^3 + 6y + x = 0$ and (3) $xy^5 - 10xy^3 + 6y + 1 = 0$. The computation determines the following values:

From (1)
$$y_0 = + 3.167, \quad - 3.167$$
$$\frac{dy_0}{dx_0} = - 0.100, \quad + 0.090$$
$$\tfrac{1}{2}\frac{d^2y_0}{dx_0^2} = - 0.008, \quad + 0.008;$$

From (2)
$$y_0 = + 0.775, \quad - 0.775$$
$$\frac{dy_0}{dx_0} = + 0.107, \quad + 0.060$$
$$\tfrac{1}{2}\frac{d^2y_0}{dx_0^2} = - 0.006, \quad + 0.016;$$

From (3)
$$y_0 = + 0.166, \quad \frac{dy_0}{dx_0} = - 0.007.$$

The roots of the given equation are $y_1 = + 3.05$, $y_2 = - 3.06$, $y_3 = + 0.87$, $y_4 = - 0.69$, $y_5 = - 0.17$.

$$(b)\quad x^4 + 4x^3 - 4x^2 - 11x + 4 = 0.$$

Here the terms to be underscored in addition to the first and last are probably the second and fourth, but as the ratio of 4^3 to 11 is rather small, it is safer to transform the equation into another lacking the second term by the substitution $x = y - 1$. There results

$$y^4 - \underline{10y^2} + \underline{5y} + \underline{8} = 0.$$

The terms to be underscored are the first, second and last and the roots are obtained by applying the method to

$y^4 - 10y^2 + 5xy + 8x = 0$ and $xy^4 - 10y^2 + 5xy + 8 = 0$. From each of the two equations two real roots, one positive and one negative, are found.

$$(c) \quad 7x^4 + 20x^3 + 3x^2 - 16x - 8 = 0.$$

Here the terms to be underscored are probably the first, second and last, indicating the existence of two imaginary and two real roots, one positive and one negative. All doubt is removed by transforming by $x = y - .7$ into

$$\underline{7y^4} + .4y^3 - \underline{18\,42y^2} - 1.404y - \underline{.5093} = 0.$$

The transformation $x = y - .7$ is selected because it is a simple transformation which makes the coefficient of the second term very small.

$$(d) \quad x^5 + 12x^4 + 59x^3 + 150x^2 + 201x - 207 = 0.$$

Here probably only the first and last terms are to be underscored, indicating the existence of four imaginary roots and one real positive root. Transforming by $x = y - 2$, which makes the coefficient of the second term small,

$$y^5 + 2y^4 + 3y^3 + 4y^2 + 5y - \underline{321} = 0.$$

The roots are found by applying the method to

$$y^5 + 2xy^4 + 3xy^3 + 4xy^2 + 5xy - 321 = 0$$

$$(e) \quad x^4 - 80x^3 + 1998x^2 - 14937x + 5000 = 0.$$

Here probably every term should be underscored, indicating four positive real roots. Transforming by the substitution $x = y + 20$,

$$y^4 - 402y^2 + 983y + 25460 = 0.$$

Here the terms to be underscored are the first, second and last. More rapidly convergent series are found by reversing the last equation,

$$25460v^4 + 983v^2 - 402v + 1 = 0,$$

and making the substitution $v = z - .01$, whence

$$\underline{25460z^4} - \underline{35.4z^3} - \underline{416.214z^2} + 8.23306z + \underline{.9590716} = 0.$$

When z has been computed, x is found from

$$x = \frac{2000z + 80}{100z - 1}.$$

Only linear transformations which make the coefficient of the second term of the complete equation or of the equation reversed zero or small are used, as other transformations become too complicated to make the method practicable.

IV.—Transcendental Trinomial Equations.

Let an equation of the form $y+af(y)+b=0$, where $f(y)$ is a transcendental function, be called a transcendental trinomial equation. Such equations are readily solved by the method, provided the resulting series is rapidly convergent, but in the absence of a transformation which insures rapid convergence the method has little practical value.

Suppose the equation $2y+\log y - 1000=0$ to be given. Applying the method to $2y+x\log y - 1000=0$, if the Napierian logarithm of y is taken, $y_0=5000$, $\frac{dy_0}{dx_0} = -4.30625$, $\frac{1}{2}\frac{d^2y_0}{dx_0^2} = +0.000215$, and $y=4995.69$; if the common logarithm of y is taken, $y_0=5000$, $\frac{dy_0}{dx_0}=-1.84948$, $\frac{1}{2}\frac{d^2y_0}{dx_0^2} = +0.00018$, and $y=4998.15$.

V.—Expansions.

If $y=z+v\varphi(y)$, where v and z are independent variables, Lagrange's series expands any function of y in powers of v. These expansions may be obtained by writing $y=z+vx\varphi(y)$ and expanding $f(y)$, which now becomes a function of x, by Maclaurin's series and making x unity in the result.

The method will be illustrated by obtaining two expansions which occur in theoretical astronomy. From the equation $E=M+e\sin E$, where E is the eccentric anomaly, M the mean anomaly and e the eccentricity of the orbit, it is necessary to find E and $(1-e\cos E)^{-2}$.

To find E, write $E=M+ex\sin E$, whereby E becomes an implicit function of x. Differentiating twice in succession with respect to x,

$$\frac{dE}{dx} = e\sin E + ex\cos E\frac{dE}{dx},$$

$$\frac{d^2E}{dx^2} = 2e\cos E\frac{dE}{dx} + ex\cos E\frac{d^2E}{dx^2} - ex\sin E\left(\frac{dE}{dx}\right)^2.$$

Making x zero, $E_0 = M$, $\frac{dE_0}{dx_0} = e \sin M$, $\frac{d^2E_0}{dx_0^2} = 2e^2 \cos M \sin M$.

Substituting in Maclaurin's series and making x unity,

$$E = M + e \sin M + \frac{e^2}{2} \sin (2M) + \ldots .$$

To find $(1 - e\cos E)^{-2}$, write $E = M + ex\sin E$ and $y = (1 - e \cos E)^{-2}$. Since y is a function of x through E,

$$\frac{dy}{dx} = -2e\,(1 - e\cos E)^{-3} \sin E \frac{dE}{dx}$$

$$\frac{d^2y}{dx^2} = 6e^2\,(1 - e\cos E)^{-4} \sin^2 E \frac{dE}{dx}$$

$$- 2e\,(1 - e\cos E)^{-3} \cos E \left(\frac{dE}{dx}\right)^2$$

$$- 2e\,(1 - e\cos E)^{-3} \sin E \frac{d^2E}{dx^2}$$

Placing $x = 0$, when $E = M$, $\frac{dE}{dx} = e \sin M$ and $\frac{d^2E}{dx^2} = 2e^2 \sin M \cos M$,

$$y_0 = (1 - e \cos M)^{-2},$$

$$\frac{dy_0}{dx_0} = -2e^2\,(1 - e \cos M)^{-3} \sin^2 M,$$

$$\frac{d^2y_0}{dx_0^2} = 6e^4\,(1 - e \cos M)^{-4} \sin^4 M,$$

$$- 6e^3\,(1 - e \cos M)^{-3} \sin^2 M \cos M.$$

Substituting in Maclaurin's series and making x unity,

$$(1 - e \cos E)^{-2} = (1 - e \cos M)^{-2} - 2e^2\,(1 - e \cos M)^{-3} \sin^2 M$$
$$+ 3e^4\,(1 - e \cos M)^{-4} \sin^4 M$$
$$- 3e^3\,(1 - e \cos M)^{-3} \sin^2 M \cos M + \ldots .$$

In like manner all expansions obtained by Lagrange's series may be obtained by a direct application of Maclaurin's series. Of course it is evident that if e is considered a variable the derivatives with respect to e may be formed and the introduction of x is unnecessary.

Historical Note.

Lagrange, in the memoir "Nouvelle methode pour resoudre les Equations Litterales par le moyen des Series," read before the Berlin Academy in 1770, found all the roots of an equation in infinite series. McClintock, in Volume xvii of the *American Journal of Mathematics*, obtained by his Calculus of Enlargement series better adapted to computation. It was recognized that these series may be obtained by Lagrange's series. McClintock calls the coefficients of the terms which have been underscored the dominants of the equation. The method of the present paper brings the computation of the roots of equations by means of series within the range of elementary instruction.

Since completing this paper the author found in an extract of a letter from Cauchy to Coriolis, of January 29, 1837, published in the *Comptes Rendus* of the Paris Academy, an announcement of important results to be obtained by breaking up an equation into two parts and introducing as a factor a parameter into one part, which parameter is ultimately to be made unity. In a postscript Cauchy states he discovered the advantage of making one part a binomial. But the author has been unable to find the method sketched in this letter developed. It would indeed be surprising if a method so strikingly direct had escaped notice.

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ON THE PROPERTIES OF THE FIELD SURROUNDING A CROOKES TUBE.

BY ARTHUR W. GOODSPEED.

(Plates II and III.)

(*Read May 15, 1903.*)

The investigation of the subject implied by the title of this article was suggested by the unexpected presence on some radiographic records of peculiar markings outlining certain bodies *below* the plate, in addition to the expected shadows of the objects above the plate—*i.e.*, between the sensitive film and the vacuum tube.

While using an iron tripod stand with a ring-shaped top as a support for a radiographic plate, it was noticed that the plate when exposed to X-rays seemed to be influenced locally by the presence of the iron ring below. For after exposing a circular piece of bronze placed on the upper side of the plate which had rested on the stand during exposure, the development showed that just above the metal of the stand the plate was appreciably less affected through the bronze than under that portion of the latter which had not been over the metal support. This startling observation suggested at once more careful investigation, especially since on first thought it would seem that if the metal below the plate could have any effect, the result should be quite the contrary to what was observed—*i.e.*, if the metal below sends off "emanations" of some sort which might produce an effect on the sensitive film, the latter would be expected to show an increased density where influenced both by the rays from above and by the emanations from beneath.

Apparent anomalies have on several occasions been noticed on radiographic plates, some similar to that just mentioned, but these have never been definite enough to invite special investigation.

A large number of experiments were made at once in rapid succession with strips and plates of various substances both below and above the sensitive film, with results always the same in character though differing in intensity of effect in different experiments and with different materials. As examples of the character of some of the tests, sheets of paraffin, mica, and of aluminum were successively placed between the under metals and the film, with the result that the effect in every case was similar, only a little less intense

than when no screen was interposed. The original records of all these experiments and the particular conditions in each case have been carefully preserved. A single figure will be enough to illustrate this effect.

Fig. 1 shows the result when two zinc blocks, one of them polished, were placed below the photographic plate upon which the latter rested. On this were a strip of copper, one of lead, a triangular and thicker piece of uranium, and a piece of metallic indium about one millimetre thick and three centimetres square. Fifteen centimetres above this combination the discharge tube was operated for twenty-five minutes, the rays being directed downward. The zinc blocks were below the lateral edges of the plate and covered each about a third of its area. There certainly is nothing ambiguous about the result, and the degree of polish seems to have nothing to do with the effect. The middle third is distinctly darker than the rest in those parts just under the metal pieces.

The transverse strip in the middle was lead and is distinctly less pervious than the copper on the left.

In looking up some of the early work of Roentgen, I found that one of his experiments was almost identical in character with those just described, but less strenuous and designed for quite a different purpose.[1] He arranged star-shaped pieces of four metals, platinum, lead, zinc and aluminum, covered by a light-protected photographic plate, film towards the stars and glass towards the tube. On development after exposure to the rays from a focus tube identical in principle with that universally used at present, the metal stars showed darker than the rest of the ground. The purpose of his experiment was to demonstrate a possible reflection from the metal stars, and the result obtained was interpreted as conclusive evidence at the time that such was the case.

For obvious reasons it seemed desirable to repeat Roentgen's experiment as nearly as possible as he made it. This was done with some difficulty, on account of the fact that the apparatus in use developed rays of such penetrating power that the glass backing of the sensitive film offered little obstruction, and even with a very short exposure the whole film was so dense as to show nothing of the metal pieces.

Increasing the thickness of the glass made it possible, after several trials and by using a contrast-developer especially prepared for

over-exposures, to produce a fairly definite result, as shown in Fig. 2. It is to be noted, however, that the parts of the film just next the pieces are *less* dense than the rest—*i.e.*, the shadows are light on a darker ground.

Fig. 3 shows the result when to the glass of ordinary thickness was added thick blocks of zinc. The characteristics of these two plates are identical, except that the latter is more dense and shows greater contrast.

In Fig 4 we have reproduced a plate made just as was that of Fig. 3, except that the exposure was thirty minutes instead of fifteen. The appearance is certainly remarkable, for though the direct X-rays had been entirely cut off by the zinc blocks the shadows are exactly as would have been produced by reversing the process and exposing directly to the Roentgen rays, though for a much briefer time.

The influence on the side of the plate remote from the tube seems to have more than neutralized the Roentgen reflection effect, and the more so the greater the exposure.

From these three experiments it seems probable that with a much less powerful X-ray generator, a suitable exposure would show the result noted by Roentgen. As is seen below, this was probably not due to reflection.

The next plate (Fig. 5) shows the impression of the ring stand, above spoken of, when the former was covered with a sheet of copper about a millimetre thick and exposed twenty-four minutes. In Fig. 6 the stand is replaced by a brass ring supported by a block of wood. The penumbral effect around the inside edge is to be noted.

As an interesting modification of this experiment I asked one of my associates, Dr. Richards, to hold his hand beneath the plate, protected above with thick metal blocks, and exposed the combination five minutes. The result (see Fig. 7), though lacking in definition, is quite like the first radiographs made without a focus tube.

We seem now to be led up to a satisfactory explanation of what we have observed so far—*i.e.*, of this apparent "nether effect" reaching completely around into the shadow of an obstruction totally impervious to X-rays proper, and acting in a direction just opposite to that of the rays from the tube.

It must be noted here that so-called "X-ray diffusion" has long been recognized, and an early experiment[3] with a fluoroscope

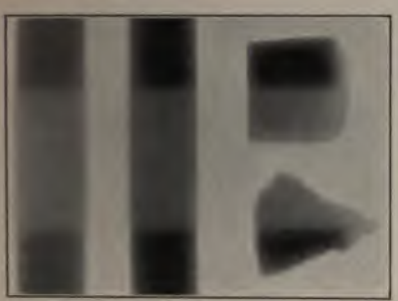


FIG. 1.

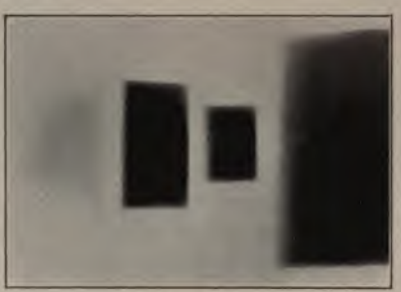


FIG. 4.

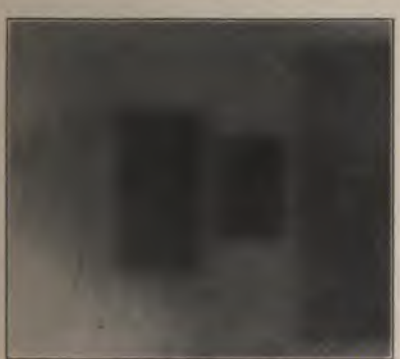


FIG. 2.



FIG. 5.



FIG. 3.



FIG. 6.



behind a thick steel plate was explained variously, the most thoughtful suggestion perhaps being by Prof. Elihu Thomson, who proposed that the screen was rendered luminous by the action of X-rays reflected from various objects in the room.

From all the experiments yet made in the effort to account for what at first seemed to be, to say the least, a paradox of science, it looks as if the whole space field in the neighborhood of a focus Crookes tube in operation is full of some sort of subtle energy, radiant possibly, but incapable of affecting the human eye, though leaving its mark on a photographic plate.

It was found by Sagnac[3] that many bodies in the path of X-rays acquire the property of emitting emanations of some sort capable of causing fluorescence and photographic action.

Undoubtedly then the effects above described are due to the secondary radio-activity of the air, the table and other bodies favorably located to be impinged by the X-rays directly.

In order to gain more knowledge of the possible limitations of this "radious" field, metal tubes of various sizes and lengths were placed on the plate, and now for convenience the entire local order of the articles used was completely reversed. Furthermore, the Crookes tube *was enclosed in a black wooden light-tight box, and all experiments were made in the night, so that every trace of optical light might be more easily excluded.* We have now the tube in its box, so placed that the axis of the ray-cone is directed vertically upwards. On the upper surface of the box and over the focus of the tube is a bundle of lead plates about one centimetre thick. On this, film upward, is the photographic plate.

This arrangement differs from that of Sagnac in that the *fluorescent light* from his tube was not filtered out, as it is here, by the box enclosing the X-ray bulb.

In Fig. 8 we have the result of a twenty-three minute exposure, when a brass tube five centimetres high, eight centimetres in diameter and three millimetres thick is placed on the plate, the tube being open at the top. This experiment was repeated with a thick block of pine wood, placed on top of the brass tube, with no change in result. The condition of the enclosed space is independent of the presence of the wood, and the enclosed area of the film is much affected.

When however the tube is covered with a thick block of zinc, this seems to protect the sensitive film completely from outside

influence, for the density of the exposure was found to be the same over the area within as under the edge of the tube, *i.e.*, nearly zero.

It does not seem possible that this effect could result entirely from the action of the Sagnac rays, since little if any of the area at the base of the brass cylinders can be reached by a straight line from any particle of matter traversed by the direct X-rays. It can be explained as a tertiary effect, produced by the air or wood just over the top, which had received its energy from the secondary emanations of other bodies in the direct path of the X-rays, or possibly the secondary or Sagnac rays may be of the nature of dark phosphorescence, *i.e.*, lasting for a time after the cause has ceased. Reasons for favoring the latter view appear as a conclusion to this paper. In this case the diffusion of the air in the room would cause the whole space to be uniformly active.

The arrangement just described suggested some easy tests on reflecting or diffusing power of different surfaces, as Sagnac had made in a different way in his investigations. In a brass tube similar to the one used above, at two points 90° apart, windows were cut 1 centimetre wide and 4.5 centimetres high. This tube was capped with zinc or lead, so that nothing could enter except through the windows. It was placed on the plate and a polished zinc block arranged opposite one window. Fig. 9 shows the result, all other conditions being as before. The exposure was twenty minutes. The streak entering the window opposite the zinc is unmistakable, and the diffused "radious" state of the whole enclosed space is demonstrated by noting the line of contact of the tube. A little brush in at the other window, too, is clearly distinguishable though faint. It seemed most desirable now, if possible, to make this phenomenon optically visible, and with this in view the following arrangement was set up:

Instead of the smaller lead block used with the radiographic plates, sheets aggregating one centimetre in thickness and a little larger than a 7x9 screen were placed on the box. On this a barium platinum cyanide screen was placed, face up, but covered with a piece of pasteboard. In this cover a circular hole was cut just the outside diameter of the window tube described above, through which the latter was placed, resting on the fluorescent screen. Its length was doubled by placing an extension on top. This was found by experiment effectually to exclude all noticeable influence except that through the windows.



FIG. 7.



FIG. 8.



FIG. 9.



FIG. 10.



FIG. 11.



FIG. 12.

The whole was in a perfectly dark room optically, and the eye was placed above the tube looking down. After the eyes had acquired a maximum sensitiveness by the total exclusion of light for ten to fifteen minutes, the Crookes bulb was set in operation and the space within the brass tube critically examined from above. The screen was unmistakably luminous to the eye and the windows were clearly located. Now the polished zinc was moved about in front of one of the openings, in the hope of detecting a variation of luminosity on the screen opposite this window. The result was at first disappointing; the position of maximum effect was certainly not that of 45°, as employed in the photographic experiments. In fact, very inconsistent positions seemed to give the greater illumination through the window under attention. Finally it became quite obvious that the zinc had little to do with what was visible. In fact, on laying aside the metal I was able to light up brighter than ever the inside of the brass box by holding my hand in a suitable position in front of the window.

This experiment made certain by ocular demonstration that the human hand has by being placed in the path of the X-rays absorbed some sort of energy, by means of which it has acquired the property of emanating something capable of exciting fluorescence upon the screen. It remains now to demonstrate what effect these emanations will have upon a photographic plate as compared with those from the zinc, and Fig. 10 shows the result of a three-minute exposure with my hand only, placed opposite one of the windows, the tube resting upon a photographic plate in its usual protecting envelopes. A similar experiment was next tried (see Fig. 11) by holding a hand in front of each window, one of the latter being closed by a thin sheet of plate glass. It is obvious from the results obtained that the physiological rays emitted by the hands affect the plate through its protecting covers, but are unable easily to penetrate glass.

It is only a step now to produce a "physio-radiogram," and Fig. 12 is a reproduction of a record made by the secondary activity emanating from my own hand stimulated by a stream of Roentgen rays with an exposure of three minutes. The shadows are those of a cent, a gold finger-ring and a piece of aluminum about half a millimetre thick, and it is apparent that aluminum is somewhat translucent to these rays.

Although Guilloz[4] had made just such shadow radiograph-

Sagnac rays emanating from his hand, the visible fluorescence generated by the tube was not cut off by any opaque screen, and there is no reason for assuming that this light may not have played some part in his results. In the present experiments everything has been done in complete optical darkness.

I have been unable to find out if Guilloz's pictures were actually published, and so cannot compare his results in detail with my own.

In connection with the present subject, my attention has been called by unpleasant personal experience to a very suggestive coincidence. The nature and pathology of X-ray dermatitis is, and has been from the first, surrounded with mystery. Much ingenious technical literature has been published in the medical journals all over the world for the last six years, with the result that to-day little is known about either the real cause, the nature, the proper method of preventing, or the best treatment of this most distressing and lingering affliction. A comparative history of many cases reveals many inconsistencies, followed by an increased sense of ignorance on the subject. The personal experience to which I refer suggests a possible step towards a better understanding of the phenomenon.

During a week in June, 1902, I occupied the Roentgen ray room as a sleeping apartment. At the end of this time an acute inflammation of the eyes and throat appeared, all symptoms of an ordinary cold or of any digestive disturbance being absent. At the end of the week referred to I left town and the inflammation gradually subsided during the next three or four days. For similar reasons I had occasion to sleep in the same room during the first week of the present month. At the end of that time my attention was painfully called to a recurrence of the symptoms observed a year ago. On ceasing to sleep in the room all trouble disappeared.

As I have never had any such experiences other than those referred to, it seems not too much to infer that the peculiar inflammatory condition may have been due to some action of the secondary emanations sent out by the walls and air of the room after the generation of X-rays had ceased. Continuous breathing of such "darkly phosphorescing" air might well account for the trouble in the throat and vocal chords. In the daytime the doors and windows were always more or less open, so that the air was continuously changing, and my eyes were protected considerably by glasses, through which neither the primary nor the secondary rays pass easily.

The inference seems fair that the recurrence of the inflammatory condition was not a mere coincidence, and that these secondary rays may be found to be of more importance than has been supposed.

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University of Pennsylvania,
Randal Morgan Laboratory of Physics,
May 15, 1903.

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ON THE DEPENDENCE OF WHAT APPARENTLY TAKES PLACE IN NATURE UPON WHAT ACTUALLY OCCURS IN THE UNIVERSE OF REAL EXISTENCES.

BY G. JOHNSTONE STONEY, M.A., SC.D., F.R.S.

(*Read April 3, 1903.*)

CHAPTER I. INTRODUCTION.

Hitherto attempts to ascertain the events that are actually happening in the universe of real existences, and to ascertain what those existences are—in other words, the study of ontology—have been pursued almost exclusively from the standpoint of the metaphysician of the human mind. This mode of treatment has led to a few negative results which are chiefly of value by helping to dispel some popular errors, but it has established little that is positive, or that can be of service to the scientific student of nature. And yet investigations of Natural Science have been pushed in more than one direction into contact with problems of ontology, and are there brought to a stand owing to the different levels at which these two fields of investigation lie. Examples of this are met with in physiology, when we find our progress blocked on coming face to face with the problem as to what is the true nature of the interdependence between the thoughts of animals and changes in their brains; and generally throughout physics, when we make any attempt to penetrate to the causes of the events that occur. It appears, therefore, to be in an eminent degree desirable that an attempt shall be made to bring natural science and ontology into line by carrying on the ontological investigation from the standpoint of the scientific student of nature.

There are other reasons also why the inquiry should be taken up by scientific men. The difficulties which have to be encountered are perhaps not so much intrinsic as collateral. They all arise from the circumstances under which we, men, have to carry out the inquiry, and are of a kind with which scientific men are better fitted to cope than others. A very serious liability to error is consequent upon the excessively secluded position of the human mind in the universe of existing things. How indirect and how slender the connections will appear in the sequel. This creates illusions greater than those experienced by the old astronomers who were misled by man's being tied to an earth that seemed to them to be stationary. Another chief source of our difficulties is that we have to enter on this study hampered by crude beliefs in which we have been brought up, which are embedded into the language we are obliged to use, and in which we habitually think; but which the inquiry shows to be a jumble of truth and error. These we must make it our business to correct, retaining the germ of truth in each, and by slow degrees acquiring the power of amending, promptly and without effort, all those parts of these beliefs which require correction. We are far from having done enough when we merely become aware of the errors; nor is it even enough that we shall have discovered what ought to take their place. We have not accomplished our task till it becomes our second nature to do this habitually and without premeditation, with regard to all that is about us and all that is within us. This takes time. But when it is accomplished the reward is great. A special difficulty arises from our being obliged to use some one of the languages that can be understood by our fellow-men. Every language that has been devised by man implies mistaken views in ontology; and that not occasionally, for every human language is permeated by these errors.

Now, students of natural science, men who have had an extensive training in the study of nature, and especially those who have devoted themselves mainly to the dynamical and physical aspects of that study, are better equipped for contending successfully with these difficulties than are their fellow-students whose main training has been confined to the tiny plot which lies within the ring-fence that surrounds the works of man—the languages he has devised, his literature and history, his music, poetry, architecture, painting and sculpture, his jurisprudence, his moral relations, the metaphysics of his mind, and so on; in fact, all branches of what in our universi-

ties are called the humanities. Explorers of nature, investigators of the work done or being done *which is not man's work*, stand a better chance of success than those whose thoughts mainly travel within the narrower range: and on several accounts; first, because they find less difficulty in freeing themselves from the limitations of the human standpoint, which we may liken to the Ptolemaic point of view, and in grasping the wider resources of a more Copernican survey; largely, too, because they more easily become expert in using such symbols as words in a generalized or otherwise modified sense when it becomes necessary to do so; but perhaps most of all because they are already familiar with the contrast between the two kinds of supposition which those physicists who use language carefully distinguish as *theories* and *hypotheses*. As some readers of the papers I have already written on these subjects have found here their chief difficulty, it appears desirable to devote a chapter of this essay to its elucidation. This, indeed, is almost necessary; inasmuch as sound progress in the task before us is not even possible unless this distinction is clearly grasped, and unless a facility has been acquired in handling both hypotheses and theories without risk of the confusion between them which has been too often made.

Chapter 2. Of Theories and Hypotheses.

Both theories and hypotheses are suppositions—a theory means a supposition which we hope to be true, a hypothesis is a supposition which we expect to be useful. Theories accordingly are either correct or incorrect, true or false, quite irrespectively of whether we, men, can make much, or little, or any use of them. *The merit of a theory is simply to be true.* It often, indeed usually, happens that the true theory is also useful; but it by no means need be so. Accordingly, the question whether a particular theory is of any use is irrelevant.

On the other hand a hypothesis is a supposition which aims at being useful, and *which is legitimate if useful.* A hypothesis may be a theory—in other words, a supposition which we make expecting it to help us forward in our investigation, may also be the supposition which we think to be true: but it by no means need be so; and in fact the best, *i.e.*, the most useful, hypotheses are often of the kind that make no pretense to being true. For example, all applications of mathematics to the investigation of nature are de-

ductions from data, in which simpler machinery is intentionally substituted for complex operations going on in objective nature. Thus, in computing the mutual perturbations of the planets, the planets are treated as though they were spheres, made up of untextured spherical shells, each of uniform density throughout; and it is left out of account that they approach to being spheroids, with mountains on their surface, irregularities of a like kind at greater depths, rocks in those mountains, minerals in those rocks, a different molecular texture in each mineral involving numberless motions among and within the molecules; moreover with tidal strains, heat expansions by day, contractions by night, and so on; perhaps seas and an atmosphere, vegetation and animals, all in constant and complicated movement; with a multitude of other details. Now it is legitimate to omit all these from our calculation, for though every one of them produces its effect in actual nature, the difference between their joint operation and that computed from the immensely simplified hypothesis made by the mathematician, can be shown to be too small to make any approach to being detected by any human appliance. Hence, for any purpose which is of use to man, the approximation arrived at by the simpler problem is sufficient, wherever the errors are of such a nature that they are not cumulative. *Nevertheless, it should be clearly recognized that it is a model of nature—a mechanism illustrating nature—and not nature itself, that has been mathematically investigated.*[1] So it is with all dynamical

[1] This has been sometimes overlooked. A recent instance is in a determination of the rate at which gases escape from atmospheres, based on the insufficient data commonly used in the mathematical investigation of such problems, and leading to a rate for the escape of helium from the earth's atmosphere which is negatived by observation (see Bryan, on the Kinetic Theory of Atmospheres, *Phil. Trans. of the Royal Society*, vol. 196 A, 1901, p. 1; and Stoney, on the behavior of helium in the earth's atmosphere, *Astrophysical Journal*, vol. xi, 1900, p. 369).

In such cases it may be difficult, and is sometimes impossible, to put our finger on the oversight that has been made. In this instance it may be conjectured with some probability that the mistake has been in the tacit assumption that the partition of energy between the internal and the translational motions of the molecules takes place with a frequency which warrants our arguing from the supposition, tacitly made in the mathematical investigation, that it goes on without intermission. There seems reason to believe that this partition of energy actually takes place, not at every encounter, but only at encounters as infrequent from the molecular standpoint as those that make chemical reaction possible between the molecules of a mixture of suitable gases. Now this, in the case of a mixture

investigations: the data of nature are loaded with minute detail and are far too much involved; they have to be simplified to bring the task within the range of man's power over mathematical analysis.

But for our present purpose a specially instructive instance is found in Geometrical Optics. The correct objective theory of light appears to be that light consists objectively of waves of alternating electro-magnetic stresses advancing through the ether. Now the whole of Geometrical Optics—which is one of our most useful sciences—is built upon the supposition that light consists of rays—a supposition which must not be mistaken for a *theory* of light: on the contrary, this supposition is to be employed as a useful and therefore legitimate *hypothesis*. In Geometrical Optics what we investigate is the succession of events, not in nature, but in a model of nature. We have substituted a model which contains far more easily handled machinery than that which operates in nature, every step in the progress of which can be foretold by the application of singularly easy mathematical analysis, can be represented in easily understood diagrams, and can be imagined and followed without difficulty by students who possess but little skill. What a loss we should sustain if that most useful hypothesis were not available! the justification of which is that it is so easily dealt with, and that it furnishes results that are true within known limits. For example, the new machinery furnishes the correct positions of optical images, although the image itself, the geometrical image as it is called, differs in material respects from any real image. Thus, it presents us with an unlimited amount of detail, much of which must be regarded as false, because it is detail which does not exist in the images produced by nature. The hypothesis is useful *within* certain limits, but will mislead if misapplied.

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of equal volumes of hydrogen and chlorine, only occurs about once in 1000 million encounters in sunshine, and less frequently in feebler light, down to about once in 100 millions of millions of encounters, so far as the observations have been recorded. An infrequency of this kind would have but little effect at the bottom of our atmosphere, but would make the distribution of molecular speeds differ altogether from that which has been computed in that penultimate stratum of the earth's atmosphere from which the escape takes place.

Chapter 3. Of the Absolute and Relative Significations of Terms.

We may make use of Geometrical Optics for another purpose—to illustrate the variety of meanings which such words as existence, theory, hypothesis, actual, real, etc., may have. They are freely used in Geometrical Optics. They are there used in a *relative* sense, in subordination to the hypothesis that light consists of rays, which for the time being must be left unquestioned, and which we may call the master hypothesis, as it governs the use to be made of those terms. Thus we speak of *real* rays in front of a mirror, and of *virtual* rays behind it; we say that the true *theory* is that the image on the retina is formed by rays reflected from the front of the mirror, but that it is legitimate to make the *hypothesis* that they emanate from a virtual image behind. When, however, we take the wider view that light is an electro-magnetic *undulation*, we recognize that what a moment ago we called real rays are not real, but a machinery substituted for what is real in the new sense that we have now to give to that word. For we are now using the term *real* in subordination to a much wider hypothesis, viz.: the great objective hypothesis that not only do our perceptions exist temporarily, but also that each of those *syntheta of perceptions* which we call natural objects exists as a whole and persistently. And when we come in turn to recognize that this, in its turn, is not the true theory of existence, but only an eminently useful hypothesis—probably, indeed, the most useful hypothesis known to man—and when we find that we must advance a step behind it to reach the true theory of existence, then at last we reach the stage at which we may use the word *real* in its fullest *absolute* sense; if we succeed in acquiring a right to apply it to what is going on in the autic universe, the universe of real existences. Thus such terms as existence, theory, real, actual, etc., only attain their absolute, which is their fullest, meaning when applied to the events that go on in the Universe of Auta; and are to be understood in their objective, which is a relative, sense when applied to what we regard as going on in that great objective hypotheton which we call nature; and in another still more removed relative sense when used in subordination to the narrower hypothesis which we have to entertain while investigating nature by the science of Geometrical Optics. When once this is clearly understood we are warned, and in some degree forearmed,

against falling into mistakes between the various shades of meaning which the poverty of language obliges us to put up with in such terms as existence, theory, actual, real, etc. If in any context we have occasion to employ any of these terms in more than one of its permissible meanings, it may sometimes be advisable to distinguish between them in some such way as that which is familiar to mathematicians when they write a, a', a'', etc., for different quantities. Availing ourselves of this device, we may write [real], within square brackets, when we wish to make it explicit that the word is to be understood in its absolute, that is in its autic, sense; and [real]', with a dash, when the word is used in its objective, which is it principal relative, sense; while [real]'', [real]''', etc., may be used to signify the other relative meanings which the term has when used in subordination to more limited hypotheses, as when we describe the rays of Geometrical Optics as being some of them [real]'' and others virtual. The same treatment may be extended to any other terms that seem to require the precaution. It is against mistakes between the *objective* and the *autic* significations of words that we have to be most on our guard. This will become clearer as we proceed.

Chapter 4. Of Auta: and of the Meaning to be Attributed to the Word *totality*.

It may be seen from the foregoing pages that the human mind is better fitted to cope with the scientific study of what apparently occurs in nature, than with the attempt to penetrate behind nature to the causes of these appearances. To do this requires us to inquire what has been happening in the universe of real existences, and to endeavor to determine what those existences are.

In the scientific study of nature we travel along one of the great *highways* of human thought; in ontology we have to make our roads as well as to push our way along them. It is therefore all the more important that we should bring to our aid every help which the scientific study of nature can supply. The present essay is an attempt to avail ourselves of this assistance.

Let us for convenience call the real existences *auta* (τά ὄντα αὐτά) —the very things themselves. An auto, then, is a thing that really exists, and in no wise depends on the way we—human minds—may happen to regard it. Our impressions or beliefs about it may be correct or may be erroneous, but the term auto means the thing *itself*.

We may also use the term *universe* to mean the *totality* of these auta. To prevent confusion, it may be well to designate the totality of natural objects by some other name. We may call it *nature*, or *the cosmos*, reserving the term universe for *the totality of auta*. Or, if at any time the word *universe* is applied to the totality of natural objects, it may be written with a dash, the objective [universe]′, to distinguish it unmistakably from the autic [universe]. It is to be noted that here and elsewhere the word *totality* is to be understood as having a more comprehensive[1] meaning than the word aggregate. Any collection of auta, however disorderly, would be an aggregate of those auta. By their *totality* is to be understood those auta, *under one definite set of conditions*—viz.: under the conditions that actually prevail—with those mutual relations, performing those operations, undergoing those changes that actually occur.

CHAPTER 5. IN WHAT SENSE THE TERM *thought* IS EMPLOYED.

We shall want a term which is applicable to *everything* of which I or my fellow-men or the lower animals can be conscious; and as at present no word in the English language has this wide signification, we shall extend or generalize the meaning of the term *thought*, so as to make it serve. Accordingly *thought*, in the generalized sense in which we shall use it, embraces sensations, perceptions, beliefs, feelings, memories, emotions, sentiments, judgments, motives, acts of will, and so on—in fact, everything which comes within the consciousness of any animal. I shall also use the term *I*, or *the ego*, or *my mind*, to denote the totality (not the mere aggregate) of a certain group of these thoughts, which may be spoken of as *my* thoughts. Observe that the word mind is here used in one of the two significations which it has in the English language, and that it will not in the present essay be used in the other of those senses. Accordingly, in the present essay, the term *mind* will not be used to signify the 'spiritual substance' which, according to a view very widely entertained, is supposed to be in existence, as well as the

[1] In Formal Logic the *comprehension* of a term is *the collection of ideas* which are included in the definition of the term. Accordingly, the greater the comprehension of a term, the fewer will be the individuals to whom that term can be applied. This is expressed in Logic by saying that the greater the *comprehension* of a term, the less is its *extension*. For example, Spaniard is a term which has a greater comprehension and a less extension than European.

thoughts. This supposed existence, if there is occasion to speak of it, will be called the man's spirit; but his *mind*, at a given time, will mean simply *the totality of a certain definite group of thoughts* at that time.

Chapter 6. The Postulates of the Present Inquiry.

We are now in a position to present a list of the postulates upon which our further progress will be built. Almost all men are agreed that these beliefs are fundamental, and most men would add considerably to the list. The very short list here set forth has been obtained by excluding from the longer list all that on trial were found not to be necessary for our inquiry.

Postulates.

First Belief.—That my present thoughts exist.

Second Belief.—That my remembered thoughts have existed.

These two beliefs involve a third, viz.:

Third Belief.—That time relations exist.

Fourth Belief.—That minds more or less resembling mine exist in my fellow-men and in some other animals.

Observation.—By intercourse between my mind and the minds of my fellow-men I learn that they experience sensations which are closely related to those that present themselves as a part of my mind. Whence, and from much other evidence, I infer:

Fifth Belief.—That my sensations and theirs have their source in some existing thing or things which are not any part of my own present or past thoughts.

Bishop Berkeley entertained this belief as emphatically as other men. He held that sensations are produced in human minds by acts of will of a "governing spirit."

Sixth Supposition.—Another belief is freely made use of in the present essay, viz.: that my organs of sense and parts of my brain are in some way associated with the introduction of sensations into my group of thoughts.

This belief is, however, not a necessary postulate of the investigation. The argument can be stated in language which does not

include it; but the supposition is true, and therefore unobjectionable, and it is introduced thus early because without it we should be obliged to use unfamiliar forms of expression which would be less perspicuous.

With the same end in view, viz., to attain lucidity, the language of causation is freely used throughout the essay, but will be found not to involve anything beyond what is included in the fifth of our postulates until we enter on the consideration of "efficient" causes.

Chapter 7. Of Egoistic Auta, and of Sense-compelling Auta.

My own thoughts are, at all events, things that exist (Postulates 1 and 2): they at least are auta so long as they last. They are, accordingly, while they last, a part of the universe of existing things. But they are not the whole of that universe. In the first place, the thoughts of other men and the thoughts of the lower animals are also things that exist (Postulate 4). And beside all these auta there are also auta of the kind that produce effects within men's minds through their [organs of sense][1] (Postulate 5). This is a complete enumeration of auta—things that exist—so far as known to man.

The minds of my fellow-men and the minds of the lower animals may conveniently be classed along with my mind as *the egoistic part* of the universe—being the part of the universe which I am already in a position to know consists of auta of the same kind as those that make up the ego.

Auta of the other kind we may provisionally speak of as *sense-compelling auta*, in contradistinction to my mind and the minds of other men and animals, which are groups of auta that receive certain definite additions when and so long as our [organs of sense] are forced into action by sense-compelling auta. The totality of these sense-compelling auta we may, for brevity, designate *the*

[1] By [organs of sense], within square brackets, are to be understood the real existences, the antitheta in the autic universe, which cause in us those perceptions which when synthetized furnish the phenomenal objects to which the term *organs of sense* is also applicable, and which, when we have occasion to distinguish them from their antitheta, may be written [organs of sense]′, with a dash.

The antitheta are popularly imagined to be 'material substances' of the phenomenal objects: but this conception of them conveys an entirely erroneous idea, as will appear in the sequel. See also Chapter 4, above.

sense-compelling universe, which will accordingly mean the same as the sense-compelling part of the universe.

The whole universe, then, as known to man, consists of this sense-compelling universe and of the thoughts of men and animals. This division is convenient, although it is faulty from a logical point of view, since we shall find that the parts of which it consists overlap. We shall, nevertheless, make use of the distinction provisionally, for the sake of its great convenience to us, *i.e.*, to minds that consist of egoistic auta when venturing upon the study of other autic existences.

Chapter 8. Of the Communications Made to Me by the Sense-compelling Part of the Universe.

Now when I open my eyes or exercise any of my other senses, sense-compelling auta transmit messages to me through my [organs of sense]. These messages finally present themselves as parts of my mind, of *my* group of thoughts; and *in the actual form in which they arise within my mind* I propose to call them *tekmeria*[1]—signs within my mind that events are happening in a part of the universe that is distinct from my mind. Thus, when I look towards the fire in the room in which I sit, the actual existence, the sense-compelling auto, the antitheton of the phenomenal object, which in its relation to us it is appropriate to call the aitio-fire (*το αἴτιον*, that *part* of the entire body of causes leading up to anything to which we may *attribute* that thing), transmits one message or signal to me through my [eyes], viz.: what is commonly called the visual appearance of the fire. This is one tekmerion made to be a part of my mind by the aitio-fire so long as it is acting upon me. When, at the same time, I hold out my hands, it transmits a second message to me, the perception of warmth, through another of [my senses]. And it sends another tekmerion to me, another witness that it is in existence and producing effects, through my [sense of hearing], viz.: the sound of the flame playing over the coals.

Thus, so long as I am employing my senses upon the fire, some cause which is distinct from my mind, *i.e.*, which is not a part of my little group of thoughts, is in three different ways and in each of them a very indirect way, sending me what may be called telegraphic signals; and these three tekmeria become, for the time, a part of that fluctuating group of thoughts which is my mind.

[1] *Τεκμήριον*, a sign which is at the same time a proof of something.

The group of auta which includes the auta that make up the mind along with those that make up its synergos, is the true existence in the autic universe which corresponds to that natural object which we call the brain. The prevalent belief that the true existence is a 'material substance' hovering about that portion of space within which the phenomenal object appears to be situated, is an utter mistake, although it is a belief which has been handed down to us by generations of our predecessors, and in which we were all brought up. Numberless are the errors which have crystallized about the phrase 'material substance'; and the mischief that has been wrought by them may be judged from the circumstance that they have quite shut out of view the wonderful capabilities of the true autic existences, of which we get one very instructive glimpse when we find that the thoughts that are our mind are a small—a very small—part of one of them.

Superstance would be a less misleading term than substance; but it is better to cut ourselves completely adrift from all the misleading associations bound up with the word substance. When the relation between a natural object and its autic cause is under consideration, the present writer has found it convenient to speak of the natural object as the *protheton* and the autic cause in the sense-compelling universe as its *antitheton*. Using this nomenclature, the *brain* of a man is a protheton, and his *mind* + *synergos* are its antitheton. The mind + synergos are a part of the true autic universe: the brain is a part of that hypotheton which we call nature.[1]

With this imperfect treatment of the subject we must be content

[1] The labors of physiologists lead to the conclusion that no thought becomes a part of the mind of any animal without being accompanied by some change in its brain, using the word brain here to mean, not the onto-brain but the objective brain, which is a part of nature. These objective changes are motions of some kind Hence we find here an instance in which the autic anthitheta of certain motions *are thoughts*.

The above relation is often so stated as to imply that the change in the brain is in some way the cause of the thought. This is to mistake the weather-cock for the wind. What occurs in the autic universe is the cause of the appearance of change in nature, and not *vice versâ*.

Nevertheless it is legitimate for physiologists to work, as they usually do, under the *hypothesis* that it is the objective events that cause the autic; provided that they do not make the mistake of supposing that this interchange between cause and effect is theory.

until we can resume the discussion with the advantage of having learned what a natural object is, and what space relations are.

Chapter 10. Of Perceptions.

The tekmeria, the messages from abroad, as I experience them when an auto acts on me through my [senses], are more than mere sensations. To enable me to see this it is only necessary for me to direct my attention to the remarkable judgments about space relations which have annexed themselves to, and in some cases even substituted themselves for, my sensations. When I hurt my foot and when I hurt my elbow there is a difference in the sensations; and this difference my mind, largely assisted by the synergos,[1] has come to translate into *the perception of a space relation* between these two sensations, and between them and others. Thus the first pain is felt as a pain in the foot, *i.e.*, *in or about a certain position in space;* the second pain I similarly *localise*. So also with other sensations when they have come to be transformed into perceptions. The red which I now see in each coal of the fire is a sensation which seems to me of a certain shape and size, and at a certain distance from muscular sensations which I feel at the same time, viz.: the sensation of turning my head towards the fire, of converging my eyes in succession upon different parts of it, the sensation of now and then winking, and the sensation of making and maintaining the focal adjustment of my eyes: all of which latter

[1] The physiological view of these events would be somewhat as follows: the hurt foot and the hurt elbow are in communication with different regions of the brain, and the [effects]' produced in the brain are not the same in the two cases. Although part of these effects are the protheton of the thought in the mind, much more of them are the protheton of changes in the synergos; for, whatever the change in the brain has been, it *must* have included a body of molecular events and others with time-relations too rapidly varying to be the protheton of any such slowly changing auto as a human thought. These accordingly are part of the protheton of the synergos, since the brain as a whole is the protheton of that group of auta which includes both the thoughts that are the mind and those other auta that are the synergos. And as the more slowly changing events in the brain and those that change more rapidly are so interdependent that neither can be other than it is, without its affecting the other, so are the thoughts in the mind and the autic events in the synergos interwoven and they affect each other. It would also appear that in dreamless sleep, those special slower events spoken of above cease to occur within the brain. But some, at least, of the swifter ones are still present, so that at such times the whole antitheton of the objective brain consists of the synergos only.

sensations appear to me to be located elsewhere, viz.: at or near *the centre of space*, as *I* apprehend space. So also with the sensation of warmth which seems to me to be *on the surface of my hands* when I hold them to the fire. Now sensations which thus appear to occupy positions in space are *perceptions*.[1]

In such cases the perception is far from being a mere coexistence of sensations. It is the result of a very subtle synthesis, a synthesis usually of many sensations and of my mind's present and past experience, with probably other materials. My mind assisted by its synergos could not have effected this synthesis but for their inherited tendency to make it and their inherited capacity for doing so.

By the synthesis which results in my *visual* perceptions, a very remarkable co-ordination has been effected between the muscular, the tactual and the visual sensations produced in me by sense-compelling auta; an equally remarkable co-ordination between the perceptions of my own mind and the perceptions of my fellow-men and of other animals; above all a co-ordination between my own perceptions, past, present and future: which co-ordinations enable me promptly to form correct predictions and are *of the greatest service to me in regulating my acts*. Natural selection has probably helped to develop them. Of all the syntheses by which the mind assisted by its synergos succeeds in translating sensations into perceptions, that which provides us with our visual perceptions appear to accomplish the greatest and most useful transformation. The intense tendency to make this particular synthesis and the extraordinary facility with which I can effect it, are no doubt due to the frequent repetition of the process in an immense series of progenitors: and, in fact, there is evidence to show that the co-ordination, substantially as my synergos and I now make it, had been effected in my ancestors at a very remote geological period.[2]

[1] Perceptions are distinguished from our other thoughts by having relation to *two* situations in space—to that position in space which the object observed seems to occupy (or, in the case of warmth, to some situation on the surface of the body), and to that position which seems to be occupied by the portion of the brain which is affected when this particular thought presents itself in the mind. Our *other* thoughts—affections, beliefs, sentiments, motives, etc.—have relation to only one of these situations in space, viz.: to the situation in which the part of the brain affected seems to be located.

[2] Before birds were differentiated from other vertebrates. See the marvelous representation of balls within sockets or beans within a pod, each supported by a little stalk, which is found on the secondary wing feathers of the male Argus

A synthesis does not mean merely the act of collecting materials together. It means that and much more, viz.: the building up of a definite structure (*συντίθημι* includes the meaning of the Latin verb *construere* as well as of *colligere*). The completed structure may be conveniently called the *syntheton* (*σύνθετον*, the structure resulting from synthesis).

It is to be noted that these syntheta, my perceptions, while they last are auta, real existences: they are thoughts, parts of my mind. In fact, up to the present we have been dealing exclusively with auta, things that really exist, some of them non-egoistic, others of them parts of my own little group of auta. But in the next step which the mind takes—a very important step—it transcends these limits.

Chapter II. Of Hypotheta.

Hitherto we have treated of auta, *i.e.*, real existences, with as little reference to hypotheta, or supposed existences, as was found practicable. It is impossible for a student of ontology commencing his inquiry from the mental attitude in which we, men, must start, wholly to disentangle auta and hypotheta from one another in the earlier stages of his inquiry; but this becomes more and more feasible as he proceeds, until, in the end, there need be no outstanding confusion at all.

In the present chapter we direct our attention to what is probably the most important hypothesis that the human mind makes, a hypothesis of which we all make daily use, and which confers upon me and upon my fellow-men and upon other animals—in fact, upon

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pheasant. This most astonishing work of art produced, by nature, is effected by six or seven different colors or shades of color disposed in the same way in which a human artist would lay them on with his brush to produce the same effect.

Darwin, in his *Descent of Man*, has shown how Variation with the coöperation of Thoughts in the minds of the cock and hen pheasants, can account for the development of these wonderful artistic productions. The thought on the part of the cock is a desire or impulse to please the hen by an exhibition of his plumage, and the thought on the part of the hen is an appreciation of different degrees of excellence in the artistic effect achieved in the pictures submitted to her judgment. This implies that the hen bird possesses the same wonderful power that we possess of translating coloring and shading into form; which therefore was attained by our ancestors before birds were differentiated from other vertebrates, unless (which is less probable) it has been separately developed along the two lines of descent since that time.

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all the minds that consist of egoistic auta, *i.e.*, minds which are supplied with information through organs of sense—an inestimable benefit, by creating for our advantage those *supposed* existences which are called natural objects. They arise in the way described in the next paragraph: and according as we make progress in tracing out the way in which they arise, it will become obvious why they do us such inestimable service.

Perceptions—*i.e.*, sensations which appear to me to be planted out in space—are the tekmeria or messages which I receive from sense-compelling auta. Auta of this kind form a part of my group of thoughts whenever and so long as any sense-compelling auto is acting on my mind and my synergos, through my senses. But the perceptions which it creates within me at any one time are but a small part of all the tekmeria that it can send to me. Which of all the possible tekmeria shall exist at any one instant depends on the particular line of communication which is at that time open between the sense-compelling auto and me; and whenever I make those changes which are popularly described as "looking at the object from a different side," "touching it in a different place," and so on, what I do is simply to change the channel of communication without altering the sense-compelling auto. But I thereby alter the perceptions, the tekmeria which reach me from it. Now the sensible *object*—which persons untrained in the study of their own mind are apt to mistake for the cause of their sensations—is simply the result of the mind and its synergos effecting a synthesis of all these tekmeria. They cannot actually exist, except in succession; but my mind, aided by its synergos, has the power of *conceiving* them as though they existed—

1. Simultaneously,
2. Persistently, and
3. Without being any part of itself.

In this power of conception consists its power of effecting this most useful synthesis.

It is of importance to bear in mind that while I am what is called "looking at the object," one of the tekmeria, my visual perception at that time is *actual*—that is, it is in true autic existence; the rest of the perceptions which are compacted along with it to make up the syntheton are *potential*—that is, they are not at present in existence, but they can be brought into existence. When I "turn

my eyes away," none of the tekmeria are actual: they are all potential. Meanwhile the originating auto continues in existence during all this performance, and will, with certainty, reproduce the first-mentioned tekmerion if "I turn my eyes back," *i.e.*, if the channel of communication between the sense-compelling auto and me is reopened.

It thus appears that the sensible *object* is not at all made up of any of the parts of which the sense-compelling *auto* consists, but only of certain minute outlying portions of the widespread effects of its great activity, viz.: those effects which, by its activity, it can produce within me through a few narrow and tortuous passages; while at the same time most of its great activity is being expended in other directions. This clearly shows—

1. That the sensible object is not the auto; and
2. That for all human purposes my attaining a knowledge of this hypothetical existence is as useful to me as if I knew what the auto is.

It, in fact, tells me, *in a direct and in the most compendious form*, what effects the auto, under every variety of circumstances, *will produce within me;* for it is itself a structure *built up of these very effects put together.*

It is to be observed that ordinary language is throughout built upon the erroneous popular belief that the objects of the phenomenal world are existences, in the autic sense of that term; and, moreover, that they are the cause of the perceptions that come into existence when we exercise our senses. This is a mistake of the kind which is called "putting the car before the horse": it is to imagine that a structure built up out of the effects of a thing can be the cause of those effects. The sensible object is built up of perceptions instead of being the cause of them. Their cause is to be sought in the sense-compelling universe of *auta*, not in the phenomenal world of *objects*. We must always be careful to distinguish between autic or true existence and objective existence, which means forming a part of that great objective hypotheton which we call nature. We may sometimes find it convenient to distinguish between them by writing [existence] for autic existence, and [existence]′ for objective existence. Autic existence means existence in the absolute meaning of that term; objective existence

means existence in a relative sense, namely, what we are to regard as existence under the Objective Hypothesis.

Ordinary language suggests to all who use it a number of mistakes of the kind referred to in the last paragraph. It is, accordingly, apt to mislead us very much, and we must be constantly on our guard against illusions into which we may but too easily be led by the usages of common speech, and by associations which have grown up around familiar forms of expression. Illusions will be found to lurk in what are apparently quite harmless forms of expression, such as "I perceive a cloud moving across the sky"; and to get at what we are really justified in believing, it is well *diligently to practice ourselves* in converting such expressions into less misleading forms, *until we do so with facility.* Thus the foregoing statement is equivalent to—

1. I am a fluctuating group of associated thoughts, and the perception of a moving cloud is for a short time one of this group. This is an autic group.
2. The perception of a moving cloud is also a part of *another* group, in which it is joined, not with the other thoughts at present in my mind, but with all the other perceptions which the antitheton of the cloud *could successively produce* in my mind.
3. This useful hypothetical group, which may be called the objective cloud, is not the cause of my perceptions. Their true cause must be sought elsewhere, and, to give it a name, it may be called the aitio-cloud, or the onto-cloud. It is the antitheton, in the autic universe, of the objective cloud. The objective cloud is the protheton of this real existence, and is a part of the great hypotheton which we call nature.

Nature is here used to signify the totality of all *sensible objects.* This definition is in accordance with the usual acceptation of that term.

We have passed successively under review two acts of synthesis —the *synthesis of the first order*, whereby sensations are transformed into perceptions; and the further synthesis, which may be called a *synthesis of the second order*, whereby perceptions are built together into the sensible objects around us, each of which is a kind of synopton, or collected view, of materials only a small part of which

are in existence at any one time. But, in reality, these two acts of synthesis are now carried on by my mind and its synergos simultaneously and with astonishing ease and promptitude; and it is probable that the gradually acquired power to make them was developed *pari passu* in my ancestors at a very remote geological period.

The instinct which impels us to assign a position in space to sensations affects our visual and tactual sensations most, including under the latter term our muscular sensations, as well as sensations of roughness, smoothness, resistance, hardness, softness, and some others. We also perceive it conspicuously in the allied sensations of tickling, warmth, coolness, pain, and several others. We localize with somewhat less precision our sensations of taste and smell: and of all our more conspicuous sensations sound is that which we least refer to a definite position. We have less power of doing so than many other animals who are furnished with ears which can be turned so as to distinguish the direction of sound; and far less power than some nocturnal insects who, by their feathery antennæ, which are their auditory apparatus, are able to determine the direction of a sound with a precision approaching that of eyesight.[1] In man there are but slender materials for the synthesis.

It may make some parts of this and of the succeeding chapters clearer to give here a definition of the term *object*. This term might be applied to the objects of any hypothesis, *i.e.*, to the *supposed* existing things, which we are to suppose to be in existence so long as we are making use of the hypothesis. Thus, under the hypothesis made use of in Geometrical Optics, it would be intelligible to speak of rays in front of a mirror as having an *objective* existence; which would mean that they are the 'objects' of that hypothesis, viz.: what we are to regard as being in existence under that hypothesis. But it is usual to make the terms object and objective more definite by restricting them to one particular hypothesis; and unless it is otherwise specified, they will be applied in the present essay only to the objects of that *great* objective hypothesis described in the present chapter, which by 'the synthesis of the second order' supplies us with what are popularly called the natural objects about us.

[1] See Professor Alfred M. Mayer's experiments on the mosquito, in which he satisfied himself that the male insect can determine the direction of a sound within an angle of 5° (*Philosophical Magazine* for November, 1874, p. 380).

Men and dogs and other animals, and among men different individuals, are able to make this synthesis with more or less success. It is made with most success when the *conceived* perceptions, which are so large a part of the syntheton, are correct pictures in the mind of what they would prove to be if the proper measures were taken to make them in succession actual perceptions. Accordingly, the 'objects of nature' about us may appear to one man somewhat different to what they do to another. In the present chapter we have dealt with them as 'sensible objects,' *i.e.*, as the objects of nature, such as they present themselves to 'the man in the street.' In subsequent chapters we shall deal with them as they present themselves to scientific men; and it will then become apparent why the scientific objects of nature are to be regarded as constructed with more success than the mere sensible objects of uninstructed men.

It may also be noted that, while we are what is popularly described as 'looking at' or 'touching' or in other ways 'exercising our senses upon' the objects about us, these syntheta of perceptions are made up of perceptions a very small part of which are in autic existence. while the bulk of them have only an objective, *i.e.*, a supposed, existence. Thus, while I am looking at a chair or table, the sensible object is made up of my actual visual perception at that time, and of a great body of conceived perceptions which have to be joined to it to make up the whole syntheton: there is, as it were, a veneer of auto with a hinterland of hypotheta; forming, when combined, a syntheton which, viewed as a whole, is a hypotheton.

Chapter 12. Of the Physical Hypothesis.

It will be well to treat of the Physical Hypothesis next, as it is a hypothesis which is entertained and made use of, with more or less success, by all men, and not by scientific men only.

Natural science may be defined as the investigation of how nature [works]', of how and why events in nature [occur]'. In this definition we have to use the verbs *work* and *occur* in their objective sense, since what have [really] done the work have been, not the hypotheta which people the objective world, but their antitheta in the universe of real existences. The relation between what goes on in the autic universe and the events which as a consequence appear in the objective world may be likened to the relation between the

motions of a great machine and the movements amongst the shadows which the parts of the machine cast when the sun shines. If the machine moves in an orderly manner, so also will the shadows move in an orderly manner; and Natural Science is the study of these movements amongst the shadows. If we had adequate access to the machine, the best way to investigate the movements of the shadows would be to study what takes place in the machine, and from it to forecast what must happen among the shadows. But, unfortunately, though we can see the shadows, we can bring only an excessively small part of the machine under close inspection, and we have but glimpses of the rest. The only part of the stupendous autic universe which a human being can *adequately* examine is that excessively small group of auta which are the thoughts of his own mind, with the similarly small groups that are the minds of his fellowmen and of some other animals: he cannot even make any adequate study of the events that go on within the synergos which is so closely associated with his mind, and can only collect mere scraps of information as to what the real events are throughout the rest of the vast machine. As to that tiny group of auta that are one human being's thoughts, it bears somewhat the same relation to the mighty whole of the autic universe as their protheton, namely, some of the more slowly changing events within the cortex of his brain, bears to the enormous totality of motions that are going on objectively throughout the whole of nature. This makes it evident that the part of the autic universe that man can adequately examine is but one drop of an immeasurable ocean, and although that little drop is an actual specimen of the kind of things that auta are, it is very plain that we are not justified in assuming that it is a fair *average* specimen of them.

Working under these disadvantages, man (and the same is true of the more intelligent of the lower animals) has constructed the *Physical Hypothesis* whereby to enable him to form a correct forecast of the changes which will occur in nature. The physical hypothesis is the supposition *that the objects of nature can act on one another*, either directly (action at a distance) or through intervening media (which by many is supposed to be an essentially different kind of action). Now the objects of nature are syntheta of perceptions and ultraperceptions (as appears from the last chapter read along with those which follow); and syntheta of perceptions cannot be what really act. Nevertheless, it is eminently useful to carry on our investiga-

tion under the physical hypothesis that it is they which act, and to confine our efforts to tracing out what effects this action must be supposed capable of producing, and under what laws it must operate, in order that it may account for what occurs in nature.

This, however, is felt by many persons to be too abstract an attitude of mind; and, to satisfy them, and import into the hypothesis the plausibility which they demand, by relieving the fundamental conceptions of what is oppressively felt as the absurdity of supposing that syntheta of perceptions act, it is usual to supplement the syntheta by piling an aërial Pelion upon this solid Ossa, and by supposing that in addition to the *sensible object* which occupies any portion of space there is what is called *its material substance* occupying the same position, which, partly directly and partly by its motions, acts on other material substances—the ether being one of these so-called *substances*. According to this, which is the prevalent hypothesis among both scientific and non-scientific men, it is these 'substances' which travel about through space; and the sensible objects, which are what we see and feel, are supposed to accompany them in their peregrinations by reason of the way in which they, the substances, act (usually through intermediate 'substances') upon our organs of sense.

This is the usual point of view: but more careful thinkers will do well to eschew this somewhat convenient, but by no means necessary, encumbrance upon *the unadulterated process of physical investigation* which treats the sensible objects themselves, the bare syntheta of perceptions and ultra-perceptions, as though they were what bring about the changes that occur in nature; and will do well to occupy themselves exclusively in tracing out the laws that must, under this *hypothesis*, be in operation in order that the effects may be what they are.

This, the true physical hypothesis, is eminently useful and is therefore legitimate; but the addition that has been saddled upon it, that there are 'material substances' present, is unnecessary, and as it is misleading and tends to keep out of view the really existent autic universe, it ought to be discarded by all persons who wish to think clearly. This is the course which all careful thinkers should prefer, because it keeps clearly before our minds that in the Physical Hypothesis we make use of a hypothesis and not of a theory. By being thus careful we avoid the risk of throwing dust in our own eyes. It cannot be too distinctly kept in view that the

justification of the physical hypothesis is its utility, not its truth—its incomparable efficiency as a means of investigating nature. This is a matter about which it is far better, although it cannot be said to be essential, that students of Physics should make no mistake.

It is obvious that causation, in the full sense of that term, can operate only between the real existences of the autic universe, and that everything else that appears to us to take place is a consequence of what occurs there. In fact, efficient cause and autic cause are synonymous terms.

Nevertheless, it is convenient and quite legitimate for scientific men to speak of 'physical' causes, meaning thereby what they have to treat as causes when engaged in carrying on an investigation under the Physical Hypothesis.

A very useful scaffolding which helps us in building up our investigation is the introduction of *forces* between the physical cause (which is always the vicinity of some natural object) and the effect to be attributed to it under the physical hypothesis. We are thus enabled to speak of the acceleration of a stone in its fall towards the earth, either as being due to the neighborhood of the earth, or as being caused by a force of gravitation which acts on it, which force is, in its turn, regarded as brought into existence by the proximity of the earth to the stone. The introduction of this piece of intermediate scaffolding is found to be of service—

1. Because the force can be represented by a line whose length accurately represents the intensity, and whose direction accurately represents the direction, *of the effect upon the stone* of the vicinity of the earth;
2. Because the same effect upon the stone might have been due to other physical causes, as, for example, to a spring urging it forwards; in which case the same piece of scaffolding, a force represented by the same line in the same position, would occupy its place between the cause and the effect; and
3. Because the effect might have been different, while the physical cause remained the same: thus, if the stone lay on the ground, what the vicinity of the earth would have occasioned is stress between the stone and the ground.

Accordingly, by referring effects in nature to the operation of

forces, we are enabled in each case to indicate with accuracy *the intensity and direction of the effect*, without having to specify (*a*) which of several possible physical causes is the one in operation, or (*b*) which of the possible kinds of effect is that which is being produced: and this in practice is found to be an immense convenience.

Such is an outline of the principles that underlie the dynamical investigation of nature, which is the form of investigation that penetrates most deeply into its secrets.

Chapter 13. Of Space Relations.

In order to apprehend clearly how much has been accomplished by synthesis it is advisable that we should scrutinize more closely space relations, and man's instinctive judgments about them: and as these judgments are a more conspicuous factor of my visual and tactual perceptions than of others, it will be instructive to treat specially of them.

Many slight muscular and other feeble sensations accompany the use of my visual and tactual organs of sense. These obscure sensations are constantly changing while I am using those senses, and in an excessively complicated way. That out of such tangled material synthesis has been able to evolve so simple a result as my judgment about space relations is because, amid all the apparent disorder, there *do exist* [*real*] *relations* between those much varying sensations; and the syntheton which can be produced depends on what these relations are. They in turn depend on what relations exist between my [organs of sense] and the various parts of the auto which is transmitting messages to me; for it is while varying these that the sensations in question arise. Hence, finally, the synthesis which can be effected *depends on what relations prevail in the autic universe between my* [*organs of sense*] *and other parts of that universe.* We have no reason to suppose that these onto-relations as they may be called, these relations between auta, are in the least *like* space relations, the space relations being syntheta constructed out of the obscure sensations that are indirectly occasioned by the onto-relations, after these have been worked up by the mind and its synergos with memories of past experiences and other materials.

Whatever the onto-relations may actually be, *they are at all events*

a part of those conditions in the autic universe which determine whether auta can act upon auta. So much I know, because I have to adapt my organs of sense to them in order to get tekmeria; and it is in doing this that I experience the complicated sensations which have come, by reason of what has occurred in my long series of ancestors, to be synthetized for me into instinctive judgments of objective space relations between perceptions. It is evident then that my judgments about space relations are the result of a synthesis of materials which are themselves consequences of relations that prevail in the autic sense-compelling universe. It is these onto-relations, whatever they are, that have an autic [existence]: the space relations have only an objective [existence]'. This means that we are to treat these space relations *as though they existed* whenever we are availing ourselves of the great and most useful objective *hypothesis*, which supposes that not only do our perceptions exist but that the hinterland also exists which with them make up what are called the natural objects about us. But while making every possible use of this hypothesis, we ought, if we care to think clearly, to keep steadily before our minds that this is a hypothesis *to be made use of*, but not the correct theory *to be believed.*

Chapter 14. Of Motion.

We are now in a position to deal with the important subject of motion. The *appearance* of motion is an auto, a perception in my mind; and while this perception lasts it is a tekmerion, a proof to me that an event capable of producing this appearance has occurred in the sense-compelling autic universe. This event could send different tekmeria to me, according to the way I employ my senses upon it; and the syntheton formed by putting all these together is what is meant by the term *motion.* It is accordingly a part of the great objective Hypotheton which we call Nature. If we want to indicate the real occurrence in the sense-compelling universe, we may speak of it as the onto-motion or aitio-motion, meaning by these terms the autic event which corresponds to the synthetomotion in the objective world. It is an antitheton, and the synthetomotion is the corresponding protheton. The word motion, like all similar terms, is ambiguous, and in common speech it has to be sometimes interpreted as meaning the protheton in the objective world, and in other contexts as meaning its antitheton in the uni-

verse of real existences. The protheton is in fact a kind of synopton, or conjoint view, of the actual effects which are at that time being produced and of the possible effects that might have been produced, within modern men's minds, by its antitheton the ontomotion. It is necessary to describe it as a *kind of* synopton, since the materials that come in, or could come in, from abroad are modified by being worked up with materials contributed by the mind and its synergos.

Chapter 15. Of the Phenomenon, or Phenomenal Thought; and of its Relation to the Phenomenal Object.

The word phenomenon has three established meanings: 1. It is used by metaphysicians to mean thought in the mind. This is the original or at least an early meaning of the term: the other meanings are of recent date. 2. It is used to mean an extraordinary circumstance. This is the popular acceptation of the word. 3. It is used to mean any natural object or event in nature. This is the meaning attributed to it in works on Natural Science.

For the sake of convenience it is well to assign a name to my thought about an object of nature, or as it is often called a phenomenal or sensible object. My thought about it we may call the phenomenon, or phenomenal thought, availing ourselves of the first of the above meanings of this term. Accordingly the phenomenon within my mind at any particular time consists of all or some of the following:

1. The actual perceptions which at that time the sense-compelling auto is producing in me, if there are any such perceptions existing in my mind at that time.
2. My memory of the perceptions which that auto has on other occasions produced in me.
3. My anticipation of such perceptions as I suppose it would produce in me under other circumstances.
4. Certain suppositions with respect to this group of perceptions.

This phenomenon, or phenomenal thought, is itself an auto, a part of my group of thoughts; while, in contrast to this, it is only as a hypothesis that *the object of this thought*, the phenomenal object, can as a whole be regarded as in existence. Part of it no doubt

may be temporarily in existence, viz.: so long as the sense-compelling auto which is the source of the perceptions happens to be acting on me through my senses. During this time some of the perceptions that go to make up the phenomenal object *are in actual existence*, but only as a part of my group of thoughts. None are in existence independently of the mind, nor are any of the rest of the perceptions that go to make up the phenomenal object in existence at that time either in or out of the mind. That the whole phenomenal object is supposed to be in existence and to be distinct from the mind is therefore *a hypothesis;* most useful, but not to be thought of as *the true theory*. On the other hand, the *phenomenon*, *i.e.*, my thought about the phenomenal object, while it has the advantage of being an auto, is transitory, imperfect, very variable, and almost always erroneous in some respects; depending as it does on the extent of my information and the amount of attention I give to it: while the *phenomenal object*, though a hypotheton, has in it nothing in the least shifting or arbitrary. *It is perfectly definite:* including as it must *all the tekmeria* which its antitheton, the sense-compelling auto, *does* actually *or can legitimately* create in human minds through human organs of sense. It is intended by the word *legitimately* to exclude cases of illusion, or defects that arise through imperfection of the senses. Legitimately is to be understood as meaning when every part of the line of communication is working normally and satisfactorily.

Chapter 16. Of the Phenomenal Object, with which Natural Science Deals.

It is in accordance with the signification we have given to the word [real]′, when written with a dash, that motion in the phenomenal world shall be deemed real when it is a syntheton of the actual perceptions which an onto-motion does or of the potential perceptions which it could produce by acting on human minds through human senses. But Science, in its progress, has found this definition too cramped. The definition would limit the stamp of being *real* to those cases in which man's senses are competent to act as channels of communication between the sense-compelling universe and him. Now, scientific investigation has penetrated much farther than this—even the flimsy appreciation of what goes on in nature which is necessary for man's everyday work, renders

essential some extension of the meaning of the word *real*—and accordingly the exigencies of common life, but more especially of scientific inquiry, have made an extension inevitable, so that a motion or other part of the Objective Hypotheton is still to be regarded as [real]', although too small or too rapid or in some other way unfitted by its time or space relations to be a syntheton of human perceptions, *whenever justification for this extension exists*. The objects with which the scientific student of Nature has to deal are in fact syntheta of—

1. Actual perceptions;
2. Potential perceptions; and of
3. Certain ultra perceptions, namely, those which scientific investigation does or can warrant.

By an ultra perception is to be understood what would be a perception, if our senses were more acute.

These are more than the syntheta of actual and potential human perceptions which we have called sensible objects, and to distinguish them it will be well to give them a different name. We shall call them *phenomenal objects*. They are in much closer relation to what is actually going forward in the autic universe than it is possible for the 'sensible objects' to be. This is a necessary consequence of the restricted range of the senses possessed by man, by which the amount of detail which can be present in the sensible object is limited.

Chapter 17. Of the Diacrinominal Object.

Motions are by far the most important part of the phenomenal hypotheton, as will be obvious from the following considerations. Scientific investigation has brought to light the significant facts which are described in common language by saying that men and animals receive their sensations of sound from motion in the air, of light from events in the ether which can be ultimately analyzed into motions, and in the same way their other sensations from motions somewhere in Nature. This, put into less objectionable language, is equivalent to the statement that the auta and autic events of the sense-compelling universe which produce in me the sensation of sound through one channel of communication, viz., through my sense of hearing, are such as are also competent to produce in me through other channels, namely, through my senses of sight and

touch, another tekmerion, viz.: the perception of motion; or, at least, differ only from those autic causes which are capable of producing an actual perception of motion through those senses, in the way that the autic cause of the perception of one visible motion differs from the autic cause of the perception of a similar visible motion which is swifter or slower or on a different scale.

These remarkable discoveries have led scientific men to entertain a new and very important view of nature, in which it is regarded as made up of objects each of which consists of almost inconceivably minute and swift motions. These and the drifting about in space of some *objects, i.e.*, of some *masses of internal motions*, are the whole of this hypotheton. It may be regarded as the utmost simplification of which any synoptic view of the effects produced within the human mind by the mighty march of actual events in the real universe is susceptible; and it is therefore that synoptic view of those effects which stands in closest relation to the autic causes that have produced them.

The remarkable hypothesis described in the last paragraph may appropriately be called the *Diacrinominal Hypothesis*, as it has discriminated between the various tekmeria produced within us by the autic universe, and has selected for further synthesis one special group—our perceptions of motion—on the ground that it, and that it alone, is able *by itself and without being mixed up with other tekmeria* to people Nature with objects which are *complete* as bodying forth in a collected form the information sent us by the real auta of the actual sense-compelling universe; and which, owing to their simplicity, stand in a closer relation to those auta than the more complex objects of Phenomenal Nature. Phenomenal objects are bright, warm, hard or soft, colored, sweet or bitter, and so on; as well as moving or at rest. In diacrinominal nature motions take the place of all these. An attempt was made to give a summary of the results of this hypothesis in a Friday Evening Discourse, delivered before the Royal Institution of Great Britain in 1885, and printed in the *Journal* of that Society, so that it is the less to be regretted that it would make this paper too long to dilate upon them here. We may therefore pass at once to the consideration of the last circumstance which it seems necessary to make clear in order that we may be at length in a position to understand how the scientific study of objective Nature stands related to the real

existences and real activities of which the true universe actually consists: which is the problem we proposed to investigate.

Chapter 18. Of Physical Causes and of Efficient Causes.

Causation, in the full sense of that term, implying efficiency in the cause, can only prevail in the operations of auta.

When an efficient cause operates within the sense-compelling universe, it produces some change therein, and if this change be such that the sense-compelling universe can produce one set of effects within human minds before the change and another set after the change, then will the hypothetical existence which we call nature also undergo a change. This is because nature before the change is the syntheton made by fitting together the former set of possible effects, and nature after the change is the syntheton of the latter set of possible effects. We may liken the sense-compelling universe to a mighty machine, and nature to a shadow cast by it in a very special way. If the machine is set in motion and changed from one position to another, it produces one shadow before the change and another after; and if the change in the machine has followed a definite order, the second shadow will succeed the first in a corresponding orderly sequence: but the relation between the shadows is not the relation of cause and effect.

Accordingly, in the laws of Nature which have been discovered by scientific investigation we find abundant instances of unfailingly concomitant events and of uniformities of sequence, but not a single instance of genuine cause and effect. The so-called Physical causes are not causes in the full sense of that term. We might write them as [causes]′ with a dash, but not as [causes] without one. Nevertheless it is *legitimate as an hypothesis*, to treat them as though they were causes when and so long as we are engaged in making use of the Objective Hypothesis. If a stone be allowed to drop in the vicinity of the earth, its downward speed is accelerated by a perfectly definite law. In this case the vicinity of the earth to the stone and the acceleration of the stone's vertical velocity are two unfailingly concomitant events. This is one of the Uniformities of Nature which scientific inquiry has brought to light. But *within the domain of Physics* there is no *cause* for the acceleration. To reach the cause we must travel beyond the hypothetical domain of Physics and study the events that have taken place in the universe of real

existences. If we confine our view to Nature, the facts as to what occurs can be observed; the circumstances under which they occur can be investigated; similar cases can be compared; and the laws to which the simultaneous or successive events conform can be brought to light. But here the knowledge conveyed to us by the great Objective Hypothesis ends: Physical Science has said its utmost.

Now all this is changed when we turn to the only field of observation accessible to us in which we are dealing directly with auta. The thoughts of which I consist, the thoughts that are my mind, are auta: no doubt a very small group of auta in the stupendous totality of all auta, but still *an actual sample*, although a very special and perhaps one-sided sample, of what auta are. In the operations that go on in my mind I do find instances, some few instances, of causes producing effects. The familiar case of a geometrical demonstration producing in a man's mind a belief in the truth of the conclusion is a case in point. Here the understanding of the proof is *the efficient cause* of the belief in the conclusion which accompanies that understanding. A wish to accomplish something, and a knowledge of how to go about it, are part of the autic universe since they are thoughts, and they are a part of the efficient cause of subsequent events in the autic universe, unless counteracted by other causes. A few other examples can be obtained from the same small field of investigation: and this is all that man, in his isolated position, has any right to expect; for the bulk of his thoughts are due, at least in large part, to autic causes which lie outside his mind, either in the synergos or beyond it in the sense-compelling part of the universe; and it is there also that those of his thoughts that are known to be causes usually exhibit their effects. When perceptions or when memories arise in my mind, the effect is indeed within my mind, but the cause lies beyond it; and when 'I move my muscles,' the cause is within my mind, but it is outside my mind, upon the antitheta of those muscles, that it operates. The instances are indeed few where the causes and the effects are *both* within my tiny group of auta, and it is only in these few cases that I can have the process of causes producing effects under my inspection.

But since cases can be cited, however few, they suffice *to establish the fact* that the relation of cause and effect in its full sense

does exist in some instances in the autic universe; whereas it ha nowhere any place within the domain of physical science. I ar even under the impression that every event which has occurred i the real universe, every change that has taken place there, has been as a matter of fact, brought about by true adequate causes; althoug I am bound to admit that man lives too secluded from the rest o the universe, and with channels for communicating with it that ar far too indirect, for me to be entitled to dogmatize and to say t myself or my fellow-men that I absolutely know this to be so. A the same time it recommends itself to my mind as intrinsicall probable; and it is supported by direct evidence which makes i seem to me *probable* in a high degree—

1. Since there are some instances in which the whole proces of causation operating among auta can be observed;
2. Since no instance can be found in which observation i possible, and in which it does not prevail; and
3. Since the alternative supposition appears to be improba ble. The only alternative is that, while the few change among auta which can be investigated are found to be du to adequate causes, the rest, or some of the rest, which w cannot investigate are uncaused.

All men experience within themselves what is called the freedo of their Wills; and this may by some be regarded as presenting a exception to the second of the above statements. But no amour of introspection has enabled me to detect any exercise of my Wi which had not been caused by some motive, *i.e.*, by a thoug which forms one of my group of thoughts, or else by some inhe ited or acquired habit; that is, by the intervention of my synergo This shows me that what I describe as the freedom of my Will do not exclude adequate causes.

It is noteworthy that statistical inquiries have revealed to us th fact that averages taken over great numbers of the acts due to th free exercise of the Wills of human beings, conform to definite law This suggests that a corresponding freedom of the Will may preva throughout the mighty Autos—the totality of all auta—and ma nevertheless, produce perceptions in egoistic minds (*i.e.*, in min supplied by the Autos with information through organs of sens of such a kind that these perceptions when synthesized into th objects of nature exhibit that orderly sequence of events which w

find in nature. For this, it would be only necessary that perceptions should be caused in us not by individual events in the mighty Autos, but by vast swarms of such events operating together and producing in us an average effect. And this, on other accounts, seems to be the case (compare, for example, the significant slowness of human thoughts with the swiftness of molecular [events]'). If this view be correct, what are known to us as the [Laws]' of Nature are an outcome from [Laws] of averages among auta. To attempt to penetrate farther lies beyond the scope of the present essay. We must not be tempted to engage here in the study of the little that man is competent to learn about the individual events that are in progress amongst the auta of the sense-compelling part of the universe, or the efficient causes that operate there.

Chapter 19. Recapitulation.

What has been chiefly learned in the foregoing pages is: 1. That the objects of nature are syntheta of perceptions; and 2. That there is no warrant for our assuming that the true autic cause of human perceptions, or of the events that occur among the objects of nature, are in the least like those objects. On the contrary, every evidence that we can collect points to the conclusion that the true source of the perceptions of our egoistic minds, and of those events in nature which are usually attributed to an interaction of the objects of nature upon one another, is in reality as utterly unlike those objects of nature as the thoughts of a man are unlike the events within his brain associated with those thoughts.

These considerations when followed up lead us to reject the common belief in 'material substances' as erroneous, and it is moreover found to be misleading. It is an error which blinds the minds of those who entertain it to the stupendous Autic Universe, which is what really exists, and which transcends the supposed material universe as much as do the boundless range and vast variety of the thoughts of a human mind altogether differ from and infinitely transcend that selection of movements within the brain which accompanies those thoughts.

A theory of existence, such as that which we have sought to expound in this essay, is to be judged, not by the use we are able to make of it, but by its truth. At the same time this theory is far from being useless to the thoughtful student of nature. It becomes

available just at those points where the assumptions usually made by scientific men leave us in the lurch—as when we are brought face to face with the problem of the true relation between a man's thoughts and the events in his brain associated with them; or when the problem is to ascertain of what kind are the true efficient causes of those events that occur about us in nature.

Appendix.

In the foregoing pages the author has freely used passages extracted from others of his writings, altering them and adding to them so as to obviate, as much as in him lay, difficulties which have been felt by some of the readers of those preceding papers; and his hope is that none of these difficulties will be felt in reading the present essay.

The attempt has been made to keep to that one special path through the territory opened up to us by the study of ontology, which pursues its way among the topics of most use to us as scientific students of nature. But much may be learned by other excursions into this great field of exploration, and they end in presenting us with a spectacle of unsurpassed sublimity.

It may be well so far to trespass upon this new ground as to mention some results of the further inquiry. In certain parts of the new territory we have to venture on less firm ground than that which we have trodden in the preceding essay, and must be content with results arrived at with probability. It is then found that such evidence as can be brought to bear appears to tend with considerable emphasis to the conclusion that not only the auta that are our minds are thoughts, but that the same is true of the auta that are our synergos. Now the mind and its synergos are, when taken together, the antitheton or true autic existence corresponding to the objective brain. A similar conclusion is indicated with regard to the rest of objective nature. The antitheta of the objective events —the true autic events which correspond to them—seem, with a considerable degree of probability, to be essentially thoughts; most of them no doubt with vastly different time relations to those of the thoughts that are the human mind, but still in several material respects not unlike them. If this view is correct, the only things that [really] exist are thoughts, and the effects produced by

thoughts upon thoughts; and the laws of averages spoken of above in Chapter 18 are part of a much greater group, viz., the Laws of Thought in general, which if this view is correct are the real ultimate laws of the real universe. It will of course be seen that the laws of thought here spoken of are different and altogether beyond that paltry little group—the laws of human thought—to which they stand related much in the same way as does the whole science of dynamics to the laws of the movements of a watch.

Egoistic thoughts, such as those of the human mind, must be related in the way that we call being within the same consciousness, in order to be able to influence one another. The understanding of the steps of a proof by my mind does not produce any perception of the truth of the conclusion in another mind. The effect and the cause must both be within a group of thoughts that fall within one consciousness. Starting from this, and collecting all the evidence available, we are ultimately led to the conclusion that the Autos, the totality of all thought, is a universal mind, meaning by a mind thoughts related to one another in the way that is described by saying that they are within one consciousness. This, if true, is a very pregnant conclusion, leading on further study to very important results.

Again, the perceptions produced within egoistic minds by sense-compelling auta are an exceedingly trifling part of the great march of autic events, whence but little would be lost out of the great procession if they were discontinued, as would happen if such minds as those of men and animals ceased to be produced. With them, however, the whole 'material' universe, the great objective hypotheton, would come to an end. Similarly, it was created, not at once, but gradually according as the minds that consist of egoistic thoughts by degrees acquired the power of transforming sensations into perceptions, and the power of synthesizing the perceptions into the objects of nature.

Similar reflections meet us at every turn while we are engaged in prosecuting the further investigation; but it would lead us too far from the immediate object of our essay to refer further to them in this necessarily desultory way.

The inquiry on which we have had to enter may be approached either in the skeptical or in the scientific frame of mind. These are not only different but opposed. The motive which rouses the scientific man to exertion is his earnest desire for the increase of

which Peripatus arose, with two lines of descent, one ending in the Chilopoda and Insecta, the other in Diplopoda, Pauropoda and Symphyla, the branch finally ending in the Arachnoidea. He thus divides the Arthropoda into Branchiata (Crustacea) and Tracheata. He quotes Plate,[1] who in 1889 considered that Crustacea and the Tracheates followed each an "entirely separate developmental path," since he derived the Crustacea from the Rotatoria, and the Tracheata from the Annelida.

In 1883 Kingsley[2] inquired whether the group Arthropoda is a natural one, calling attention to the fact that the insects have been derived from Peripatus, while the Crustacea "had an ancestor resembling the Nauplius of the Phyllopoda or the Copepoda." In 1894[3] he divided the Arthropoda into three subphyla: I. Branchiata; II. Insecta or Antennata, and III. Diplopoda, rejecting the old grouping into Branchiates and Tracheates (though retaining the Branchiata), and he states his belief that the three divisions he makes "are but remotely related to one another, and it may yet be proved that they have no common ancestor nearer than the Annelids."

Indeed, as early as 1886, A. C. Oudemans[4] thus expressed his views as to the relations of Limulus with the trilobites, and of the derivation of the scorpion from the Eurypterida: "Though some zoologists doubt the relationship of Limulus with the Trilobita, the paleontologists have long ago been convinced of it. Among the numberless Trilobita there occur all possible transition forms between them and Limulus, and to Scorpio the Eurypterida form a partial bridge." His genealogical tree represents the Xiphosura as originating from the trilobites and the scorpions as derived from the Eurypterida, in this respect theoretically anticipating the results attained by Pocock with Palæophonus. Oudemans also acknowledges the close resemblance of trilobite larvæ to that of Limulus.

[1] "Ueber die Rotatorien fauna des bottnischen Meerbusens, nebst Beiträgen zur Kenntniss der Anatomie der Philodiniden und der systematischen Stellung der Räderthiere" (*Zeitschrift f. Wissen. Zoologie*, xlix, December, 1889).

[2] *American Naturalist*, xvii, p. 1034, 1883.

[3] *American Naturalist*, xxviii, pp. 118 and 220, 1894.

[4] "Die gegenseitige Verwandschaft, Abstammung und Classification der so" gennanten Arthropoden" (*Tijdschr. d. Nederland. Dierk. Vereen*, 2e Ser. Deel 1, 1886).

I also stated in 1893[1] that there are four lines of development in the Arthropoda (throwing out for the present the Linguatulina and Tardigrada), viz.: "the Podostomatous line, the first to be struck off from the Annelidan stock (the trilobites being the first forms to appear); second, the Arachnidan line; third, the Crustacean line, nearly coeval with the first or Podostomatous; and the fourth, the line culminating in Myriopods, Scolopendrella and insects; and it is safe to suppose that the terrestrial tracheate groups of Arachnida, Myriopoda and insects were later products than the marine, aquatic branchiate classes—*i. e.*, the Podostomata and the Crustacea."

Afterwards in 1898, in my *Text-Book of Entomology*, as a result of the memoirs of Lankester, Kingsley, and the work of Kishinyoue on the embryology of the Japanese Limulus, from morphological and embryological data, having abandoned earlier opinions as to the Crustacean affinities of Limulus, I gradually was led to recognize the close affinity of the Merostomes and Arachnida, stating that the embryology of Limulus and Arachnida "shows that they have descended from forms related to Limulus, possibly having had an origin in common with that animal, or having, as some authors claim, directly diverged from some primitive eurypteroid merostome" (p. 6). Again, on p. 8: "The Arachnida probably descended from marine merostomes, and not from an independent annelid ancestry." Again, on p. 3, in a discussion of the relation of insects to other Arthropoda: "It is becoming evident, however, that there was no common ancestor of the Arthropoda as a whole, and that the group is a polyphyletic one. Hence, though a convenient group, it is a somewhat artificial one, and may eventually be dismembered into at least three or four phyla or branches."

Subdivision of the Arthropoda into five Phyla.—I would suggest the following grouping of the principal classes of the Arthropoda, beginning with what may be regarded as the most primitive assemblage of classes, and for which I would propose the name *Palæopoda*, in allusion to the very primitive and homonomous nature of their post-antennal or post-oral appendages, when compared with those of

[1] "Further Studies on the Brain of Limulus Polyphemus, with Notes on its Embryology" (*Memoirs Nat. Acad. Sciences*, p. 322, 1893). Compare also *Zoologischer Anzeiger*, April 20, 1891.

the Crustacea.[1] I also add what appear to be the essential characters of the phylum.

Phylum I. PALÆOPODA. Composed of three classes—*i. e., Trilobita, Merostomata, Arachnida.*

Body trilobate (in Trilobita and many Merostomata), never protected by a true carapace, composed of a head and trunk region; the head-region separate from the trunk, in Trilobites (Triarthrus) composed of (judging by the appendages) five segments (somites, arthromeres), in Merostomes six, while in Arachnida the head fused with the so-called thorax (cephalo-thorax) also consists of six segments. The first pair of head-appendages, in a single trilobitic genus (Triarthrus), are long, slender, uniaxial and antenniform, or biramose, chelate (Merostomata and Arachnida); all the post-oral appendages, in the most primitive class (Trilobita), biramose, consisting of an outer and inner many-jointed division, but all homonomous, or retaining the same fundamental and primitive shape from the mouth to the end of the body, and never (as they are in Crustacea) differentiated into true or functional mandibles, maxillæ, maxillipedes, ambulatory uniaxial thoracic legs, or biramose abdominal limbs. The gnathobases, or coxal joint of each limb, especially those near the mouth, armed with inward projecting spines, acting as jaws to tear and to keep the food or prey from escaping. In the Merostomata the post-cephalic or trunk (abdominal) limbs biramose and adapted for swimming, and either (in Trilobites) expanded posteriorly and probably serving both for swimming and respiration, or in Merostomes (*Limulus*) bearing on the exopodite of each limb, except those of the first pair, a pile of numerous gill-sacs. In Arachnida, in adaptation to a terrestrial life, the six pairs of abdominal or trunk limbs are reduced, mostly atrophied, represented in the scorpion by the pectines and the four pairs of invaginated book-lungs, and in spiders by the two pairs of book-lungs (Mygale) and the three pairs of spinnerets, which are 2–3 jointed, external free appendages. A hypostoma is present and well developed in Trilobita and Merostomata, as also a double underlip, the chilaria of Limulus.

The eyes of Asaphus, etc., and of Limulus are compound, al-

[1] Palæocarida was proposed when I believed that Limulus and its allies were Crustacea; my name Podostomata was proposed for a group embracing the two classes Trilobita and Merostomes; the present name, Palæopoda, is needed to embrace the three classes mentioned.

ways sessile, and distinguished by the thick, either lenticular or long conical lenses, arranged in quincunx order.

The integument is chitinous, insoluble as in insects, never containing carbonate or phosphate of lime, or forming a solid crust as in the higher Crustacea. The cartilaginous plate (endosternite), so large and well developed in Limulus, is also present in Arachnida.

In the living forms (Limulus and Arachnida) the digestive canal may be differentiated into a slender œsophagus, a proventriculus armed with rows of numerous chitinous teeth (Limulus) and an intestine, the stomach being but slightly differentiated. The liver or hepato-pancreas is large and voluminous. In the Merostomes (Limulus) there are no salivary glands, though occurring in Arachnida. Genital openings always (Merostomata and Arachnida) prosogoneate, the oviducts or seminal ducts opening out separately on the posterior aspect of the basal abdominal limbs (Limulus), or in Arachnida united into a single terminal passage, opening by a single orifice at the base of the abdomen. In the marine forms, with gills or localized respiration, the heart is tubular and the arterial system remarkably developed and finely divided, whereas in the tracheate, terrestrial forms the arteries and veins are absent, respiration being carried on throughout the body (chiefly abdominal) cavity.

In the Palæopoda there is no true metamorphosis like that of the Crustacea, no nauplius or zoëa stage. The first or earliest larval stage, the *protaspis*[1] stage of Beecher, can, so far as we can see, in no way be likened to the nauplius of a crustacean. The nauplius has an oval body, not differentiated into segments, but with three pairs of slender swimming limbs, which finally become the two pairs of antennæ and the mandibles of the adult. In the protaspis of trilobites, as defined by Beecher, the conditions are entirely different and such as suggest the origin from a polymerous Annelid ancestor. The minute disk-like or suborbicular larva of different genera of Trilobites described by Barrande and by Beecher consist of two regions, a head and trunk or abdomen. There are in the head indications of five annulations, the same number as in the adult Triarthrus; the much shorter abdominal region has from "one

[1] This term was proposed by Beecher in his paper on "The Larval Stages of Trilobites" (*Amer. Geologist*, September, 1895). Previously to that, A. C. Oudemans, in 1886, in the article cited, proposed the name *Proagnostus* for the same stage. If used, this name might be amended to read Protagnostus stage.

to several annulations," which probably represent segments. From this we logically infer that in the protaspis of trilobites there were more than three pairs of head-appendages, and possibly two or three pairs of abdominal appendages. Now the larva of Limulus is hatched with two body-regions, of the same general shape as those of the trilobites, and it is also trilobed; the embryo, sometimes before hatching, with its thick spherical body, strongly recalls the protaspis stage of trilobites, and seems to justify the view that the freshly hatched larva of Limulus is a protaspis.[1] In the protaspis-like fossil Cyclus, which seems to represent an ancestral type of Limuloids persisting into the Carboniferous Period, there are traces of head-appendages like those of the embryo Limulus.

The metamorphosis of the Palæopoda is, then, incomplete; the limbs of the protaspis retain the form and functions of the larva, the adult simply differing in acquiring at successive molts additional trunk-segments, with their corresponding limbs.

The eggs of Limulus as well as of Arachnida are large and not so numerous as in some Crustacea; those of Limulus are laid in the sand. The eggs of trilobites are also large, spherical, and evidently, like those of Limulus, were deposited in the sand or sandy mud, as they occur separately from the trilobites themselves.

The embryology of Limulus presents some unique features, and yet there is such a close resemblance to that of the scorpion that the embryology of the Arachnida, as I have freely acknowledged, affords very strong proofs of their relationship to and descent from merostomes. In the embryo of the scorpion and spiders there are six pairs of head- (cephalothoracic) appendages, and the mode of origin of the book-lungs of the scorpion and spiders seems to prove that they are derivatives of the exopodites of the abdominal limbs of Limulus.

It results from what is now known of the structure of the Trilo-

[1] I freely acknowledge that many years ago (1872) I supposed that the embryo Limulus passed through a nauplius, and that I called it a "subzoëa stage," but this view was long since abandoned, as also my contention that Limulus was nearer the Crustacea than the Arachnida. It need hardly be added that while as previously I cannot agree with the view that Limulus is an actual Arachnid, it has for some years, through the result of the work of Kingsley and Kishinyoue, been evident that the Merostomes are closely related to the Arachnida, and I adopted this view in my memoir on the brain of Limulus (1893).

bita that they have no relationship with the Crustacea. To include them in that group, otherwise a most natural one, is not good taxonomy. The chief characters which are given for retaining the Trilobita as a primitive group of Crustacea are the presence of the antennæ-like preoral appendages of Triarthrus and one or two other genera. That this is not so important as might seem at first sight is the presence of four antennæ-like appendages in the head of Eurypterus; Holm having discovered that the first pair are chelate, like those of Limulus.

Both Trilobita and Crustacea have biramose limbs, adapted for swimming, but so has any marine arthropod; the fact that the limbs are divided is the result of their inheritance in either class from Annelids with parapodia, but in the multiarticulate structure of each ramus and the entire lack of differentiation of the whole series of postoral limbs in Triarthrus we have fundamental characters which are diagnostic of the Trilobites, and widely separate the class from the Crustacea. Whether the Merostomata are widely distinct from the Trilobita or not, we submit that it is a mistake to include the latter in the class of Crustacea.

Entirely disagreeing with those who widely separate the Merostomata from the Trilobites, after repeatedly going over the subject, the close relationship of the two groups seems to us to be very apparent, the differences being only such as would separate the two classes of a single phylum. It has seemed to us that the merostomes and trilobites either had a common ancestry, which was a protaspid, or the Merostomes by way of the Synxiphosura diverged from the trilobite stem after it had been established in Precambrian times. Thus far, unfortunately, we know nothing of the nepionic stage of any of the Eurypterida. Their earliest adult form (Strabops of Beecher[1]) occurs in the Cambrian, while the Synxiphosura date from the Silurian. It is not improbable that some genus of this group gave origin to the Xiphosura. On the other hand, is there not so close a resemblance between some of these Synxiphosura, such as Neolimulus and Bunodes, as to suggest that the Merostomes are direct descendants of the Trilobita? If we compare the figures

[1] Although Beecher refers this early form to the Eurypterida, it appears, judging from his figure, to quite as much resemble certain Synxiphosura, as Bunodes and Neolimulus, in the short, broad head and shape of the trunk-segment and telson, though it has two segments more than in the Synxiphosura and one segment less than in the Eurypterida.

of *Aglaspis eatoni* with that of Dalmanites (Figs. 1414 and 1331 of Zittell-Eastman's *Paleontology*), is there not such a close resemblance in the shape of the head (or cephalothorax) and of the trunk-segments as to suggest a close alliance, even though members of two distinct classes? To answer this question by saying that this is a case of convergence, the objector might be referred to the other Synxiphosura, which also suggest a common origin of the two classes from a protaspis ancestor. It has been suggested that some of the Cyclidæ are larval Eurypterida or Limuloids; if this should prove to be the case (of which we are by no means sure, as no Cyclidæ have yet been found below the Carboniferous), we should have an additional argument for the common origin of the two classes, for the Cyclidæ somewhat resemble the larval trilobites.

Relation between the Merostomata and Arachnida.—While we have from the first maintained that the Merostomes should not actually be included among the Arachnida—*i. e.*, that Limulus is not a genuine Arachnidan, as claimed by Van Beneden, Lankester and later authors—from the evidence we now have as to the mode of origin of the book-lungs and the morphogeny of the appendages in general, and especially the interesting and remarkable discovery by Mr. Pocock,[1] that the Silurian so-called scorpions are probably marine links between scorpions and Eurypterida, whatever objections I have formerly expressed to their Arachnidan affinities are now overcome, and it seems plain that the scorpion is a direct descendant of some Eurypteridan. Pocock's fortunate discovery in the Silurian scorpion (*Palæophonus hunteri*) of the inner branch of a two-jointed appendage, which appears to be the homologue of a recent scorpion's "pecten," should it be confirmed by the discovery of additional examples; as well as the thickness of the head-appendages, the last four pairs of which end in a single point, not in claws, as in modern scorpions—these discoveries appear to give the clue to the line of descent of Arachnida from some Merostome, one would say from some Eurypterus-like form, though, from other features observed by him, Mr. Pocock takes the view that "Palæophonus occupies an intermediate position between Limulus and the Eurypterida on the one hand and recent scorpions on the other, standing, if anything, rather nearer to the former than to the latter."

[1] "The Scottish Silurian Scorpion," *Quart. Journ. Micr. Sc.*, Vol. 44, n. s., 1902.

We quite agree with Pocock's opinion that Palæophonus was not adapted for land and aërial respiration, but "lived in the sea, probably in shallow water, its strong, sharply-pointed legs being fitted for maintaining a secure hold on the bottom."

In conclusion, then, we would suggest, from our present knowledge of the Palæopoda, that the group is a natural one, that the line of descent of the phylum from some Annelid-like worm was independent of that of the crustacean phylum, and that the affinities shown by morphology and embryology to exist between the Trilobita, Merostomata and Arachnida are so close that they form a tolerably definite series of interrelated classes.

Phylum II. PANCARIDA. Represented by a single class, *Crustacea.* The phylum name is proposed for the reason that the group is so well circumscribed, none but the genuine Crustacea or Carides belonging to it, forms as to whose position in nature all zoologists are well agreed.

In this group there is a decided advance over the Palæopoda in the differentiation of the appendages into from three to six kinds, with corresponding functions. In the lower or more primitive, though somewhat modified, group of Cladocera, such as Daphnia, there are two pairs of antennæ, a pair of mandibles, of maxillæ, and of legs or trunk-appendages, the whole performing four different functions; while in the Decapoda there are besides the antennæ, mandibles and maxillæ, three pairs of maxillipedes, five pairs of thoracic and six of abdominal legs, or appendages, in all performing six different kinds of functions—a degree of differentiation and specialization not exceeded by any other Arthropodan group.

The members of the phylum show an increasing tendency, as we rise towards the more specialized forms, to a heteronomous segmentation and also to a wonderful transfer of parts headwards (cephalization), the cephalothorax being covered by a carapace formed by the hypertrophy or excessive development of the tergites of the second antennal and mandibular segments. In the Phyllocarida the cephalothorax is covered by a bivalvular carapace, with a weak adductor muscle; while in Apus it, in adaptation to its burrowing in soft mud, assumes the general shape of the shield of Limulus; while in the Estheridæ the entire body is protected by the two valves, which are connected by a definite hinge and ligament. On the other hand, the head-shield of the Palæopoda, as well as the pygidium when occurring, is the result of the simple fusion of the

segments. While the Phyllopoda are generally regarded as the most primitive group—their swimming limbs closely resembling those of Annelid worms—it may be questioned whether the Phyllocarida are not a still more primitive group; certainly they are the most composite or synthetic, and were the earliest known group of Crustacea.

Crustacea are, like the Palæopoda, prosogoneate; but when we take into account the fact that there is in the adult but a single pair of nephridia (the green gland), or in other forms (Phyllopoda) the shell gland, there has been a great reduction in the number of pairs from what may have been the ancestral type, while Limulus still retains four pairs. In all Crustacea the eggs are carried attached to the body of the parent, and never, as in trilobites and merostomes, deposited loosely in the sand.

In their metamorphosis, which is a complete one in all the typical forms, the larval stage of the lower Crustacea being a nauplius, we have another feature wanting in the Palæopoda. As is well known, the early embryo of Moina passes through a prenauplian stage like that of Annelida, and the indications are that the nauplius is itself a derivation from the trochosphæra stage of Annelid worms.

Now, as is well known, the most primitive groups or members of a group do not undergo transformations; and in this respect the Pancarida are a later, more specialized group than the Palæopoda. It will be remembered that the most primitive insects (Synaptera) do not undergo a metamorphosis, and in several of the lower orders of winged insects it is incomplete, there being no larval and pupal stages; in the Arachnida only the extremely modified Acarina undergo a slight metamorphosis. That of the Meropoda is slight.

Enough has been stated, we think, to show that the Palæopoda are quite remote from the Pancarida, and that a union of the trilobites in the same class with the Crustacea brings about an unnatural association, and tends to an unnecessary amount of confusion.

Dr. Kingsley regards the Trilobita as the more primitive subclass of Crustacea, but we are unable to see any features in Crustacea which could have been derived from trilobites; there are no transitional forms, and the larval forms are widely distinct, as he has well shown. The gap between the two groups is, on morphological and embryological grounds, a very wide one. Already in the early Cambrian seas trilobites were a predominant type, while

the Crustacea were comparatively scanty in numbers, and represented by primitive types showing no trace of trilobite characters.

The Chief Factors in the Evolution of Classes.—Assuming that the Arachnida, represented by the most typical form, the scorpion, have evolved from the class of merostomes, in the way suggested by Pocock, the entire process or phenomenon has the most direct and instructive bearings on the method of evolution of one class from another.

In the first place, the single group of scorpions—say a single generic form—appears to have arisen from some genus of Eurypterida, allied to Eurypterus, and by divergent evolution the great class of Arachnida, with its eight orders, appears to have originated by one step after another from a single type, not necessarily an individual, but many, all the members of the genus being modified by similar causes, in the same manner and at the same time.

The modification of a marine Eurypteroid form, perhaps living in a shallow, land-locked basin, perhaps finally becoming brackish, into a terrestrial scorpion, was due to changes in the environment, in the topography; this reacted on the Eurypterid and resulted in change of habits, and consequent adaptation to brackish, and perhaps to fresh, water, and finally to land. With little doubt, all the forms inhabiting the area underwent the same kind of modification and similar adaptation to a new medium, the same changes of function resulting in the disuse of organs adapted for marine existence and the evolution of structures adapting the animal for terrestrial life.

The changes by which the connecting links (Palæophonus) became transformed into a genuine scorpion, the ancestor and founder of the Arachnida, were the following:

1. The loss by disuse of the abdominal swimming appendages (except the pectines), and the ingrowth and reduction by disuse of the expodites, the gills attached to them being carried in, forming eventually the book-lungs of the scorpion, each with its spiracular opening, adapted for aërial respiration.

2. The four hinder pairs of cephalothoracic appendages became slenderer after the animal had left the water and adopted a life on land, under stones or the bark of trees, etc., and the single stout claw of the original Palæophonus became by use, in climbing trees, etc., two-clawed, like those of all Arachnida and insects.

3. The compound eyes of the Merostomes became broken up into groups of single eyes.

4. Most remarkable changes took place in the internal organs, resulting in the development of salivary glands, none occurring in Crustacea and other marine Arthropods.

5. The acquisition of Malphigian or urinary tubes which exist in terrestrial Arthropods, Arachnida, insects, etc., but in no marine Arthropods.

6. A gradual reduction in the number of pairs of nephridia, all Arachnida having but a single pair, Limulus having four pairs, and the Eurypterida presumably as many.

7. After the scorpion type became fixed and the spiders arose, the number of pairs of book-lungs became reduced from two pairs in Mygale to one in other spiders, and then began an evolution of tracheæ from dermal glands—a process seen in certain terrestrial planarian worms as well as land-leeches.

8. While the arterial system of Limulus, owing to its localized organs of respiration, is remarkably developed, in the scorpion the arterial system is greatly reduced, and in the tracheate Arachnida, such as the spiders, there are no arteries or venous lacunæ.

It is most probable that the evolution of the Palæophonus descendants, viz., the scorpions of the Carboniferous—assuming that they were true scorpions—took place with comparative rapidity, *i. e.*, by tachygenesis, without the extremely slow method postulated by the natural selectionists, the modification suggested above having contemporaneously affected all the individuals, many thousands or tens of thousands alike. The method was not, as Darwin imagined, the result of a single chance variation gradually and by numberless intermediate forms passing into a species which gave origin to many others, from which were gradually evolved new subgenera, genera, subfamilies and so on, but the method was radically different. The Palæophonus, an Eurypterid, became, we take it, in a comparatively few generations the parent of a scorpion, the representative of a distinct class. The class characters, great as are the differences, especially in its internal organs, between an Arachnid and a Merostome, were assumed with comparative suddenness. New classes, like new species, did not arise from a single but from a large number of individuals. This was Lamarck's doctrine, and it has been reaffirmed by De Vries. This shows that even classes

are in a degree artificial or ideal conceptions. And so it was with the evolution of mammals from theromorphous reptiles, and of birds such as the Archæopteryx from reptiles. With our present knowledge we can trace an almost exact parallel between the tachygenic origin, by change in the medium, inducing changes in habits and the functions, of flying in sectsfrom Synapterous forms, that of the Arachnida from the Merostomes, of Amphibia from Ganoid fishes, of reptiles possibly from Amphibia like the Labyrinthodonts, of birds from dinosaurian reptiles, and of mammals from theromorph reptiles (unless the Amphibians, as some contend, were the source of mammalian life).

The exciting causes of the differentiation of classes, as well as orders, families and genera, were geological and topographic changes, enforced migration and consequent isolation, adaptation to a new medium, to new conditions of life, such as a change from marine to fresh water, from fresh water to land, and in the case of pterodactyls, birds and insects, from a terrestrial life to one spent partly in the air.

The early Paleozoic ages as well as the Precambrian were periods of the rapid evolution of phyla, and of class and ordinal types, as shown by Hyatt, the writer, and others. Indeed, it would seem as if the evolution and differentiation of varieties and species succeeded rather than preceded the formation of genera and higher groups. It may be questioned whether the natural selectionists could make any progress in evolution, so to speak, by beginning with merely simple variations, although after the higher or more general groups were originated, and this was by far the most difficult and important step, specific variations set in very rapidly, as early as Cambrian times. Few, except palæontologists, appear to appreciate the rapidity with which evolution in Precambrian and Cambrian times must have operated among the plastic forms which here and there crowded the early paleozoic seas.

Phylum III. MEROPODA. This group is proposed to include the classes of *Pauropoda*, *Diplopoda* and *Symphyla*.

Prosogoneate myriopods, in which the body is in the typical forms cylindrical, the trunk-segments variable in number, but usually numerous, and each segment "double"—*i. e.*, united by a dorsal plate, which was originally two plates which had been fused together (Heathcote), unless we adopt the views of Kenyon that the alternate plates disappeared, the remaining plates overgrow-

ing those behind them, so as to give rise to the anomalous double segments; the feet arise close together along the median line of the body, there being no sternal plates between them. In the typical Diplopoda the head consists of three segments, the preoral or antennal and two postoral, bearing the mandibles (protomalæ) and the single pair of maxillæ (deutomalæ) united to form the gnathochilarium or underlip. As all the members of this phylum agree in having from two- to three-jointed mandibles, in which respect they differ from Chilopod Myriopods and especially insects, we have given the name *Meropoda* to this phylum in allusion to the primitive nature of these appendages, which resemble the maxillæ rather than the mandibles of insects.[1] The mandibles of the Diplopods consist of three segments, a basal segment (cardo), a middle segment (stipes), and a distal one (mala mandibularis), which supports two lobes homologous with the galea and lacinia of the maxilla of an insect. Diplopods are also provided with eversible coxal glands, in position like those of Scolopendrella; these perhaps functioning as blood-gills, and in Lysiopetalum occurring between the coxæ of the third to sixteenth pair of limbs.

A primitive feature, and the one diagnostic of the Meropoda as compared with the Chilopodous Myriopods, is the paired genital ducts and openings which are situated near the head between the second and third pair of legs. In the Symphyla the opening is single, proving the later origin of that group. Another diagnostic feature is that the male genital glands lie beneath, while in Peripatus, Chilopods and insects they lie above the digestive canal.

The tracheary system is also more primitive than in Chilopoda and insects, the tracheæ not being branched (except in Glomeridæ) and anastomosing, and the tracheæ themselves are without spiral threads (tænidia). In Diplopods the stigmata, which are permanently open, are placed beneath the legs, or even in the coxal joints. The nervous system is much more primitive than in Chilopoda and insects. The external genital armature, a complicated apparatus of male claspers and hooks, apparently arises from the sternum of the sixth trunk-segment, and they are modifications of the seventh pair of legs.

In their embryology the Diplopoda are more primitive than the

[1] There is an approach to this trimerous condition in Thysanura and Orthoptera (Blatta) and certain Coleoptera (see my *Text-Book of Entomology*, p. 60, also p. 12).

Chilopoda. In Polydesmus the method of formation of the blastoderm more resembles that of the Crustaceans and Arachnida than that of Chilopods.

The larva of the Diplopods, though bearing but three pairs of legs, differs from that of any insect in that these limbs are not appended to consecutive segments; either the second or the third segment in different species of Julus being legless, while in Blaniulus the legs are borne on segments 2 to 4 behind the head.[1] The new double segments with their two pairs of legs arise at successive molts, so that the animal undergoes a partial metamorphosis; while the Chilopods are hatched in the form of the adult, being polypodous.

PAUROPODA. Nothing has been added to our knowledge of these forms since the publication of the thorough works on them by Kenyon and by Schmidt.

The group was regarded as an order (Pauropoda) by Lubbock. Kenyon, however, created the order *Protodiplopoda*, including in it Pauropus and Polyxenus.

The Pauropoda are regarded by Kenyon as degenerate Diplopods, owing to the absence of tracheal and circulatory systems, and distantly related to Polyxenus; on the other hand, the simplicity of the segmentation, the fact that there is but a single pair of legs to a segment, and other features pointed out by Kenyon, lead us to provisionally regard the group as a class more primitive and distinct from the Diplopoda. The number of mouth-appendages is the same as in the Diplopods; the genital aperture opens on the third trunk-segment, and the testis is situated above the intestine (the ovary below).

History of the Opinions regarding the Taxonomy of the Meropoda.—The first writer to doubt the naturalness of the group Myriopoda, and to state that the Chilopoda and Hexapoda were more nearly allied than the Chilopoda and Diplopoda, was Pocock,[2] in 1887. A year later Dr. Kingsley[3] arrived independently at the same opinion, adding the anatomical data in support of this view.

[1] The young of Polyzonium, however, is hatched with four pairs of legs, borne on each of the first four trunk-segments (Rimsky-Korsakow, *Travaux Soc. Imp. Naturalistes de St. Petersbourg*, xxv, 1895, Pl. 1, Fig. 8).

[2] *Annals and Mag. Nat. Hist.*, xx, October, 1887.

[3] *American Naturalist*, December, 1888, p. 1118.

In 1893 Pocock[1] divided the tracheate Arthropods into two sections, the *Progoneata* (including class Pauropoda and class Diplopoda), and *Opisthogoneata*, embracing the Homopoda (class Symphyla and class Chilopoda) and Hexopoda (class Hexopoda). Afterwards[2] he placed the Symphyla among the tracheate Progoneata.

This classification of Myriopoda was adopted by Schmidt in 1895, and has been adopted by Verhoeff[3] and the term Myriopoda will probably hereafter be merely used as a convenient appellation for polypod tracheate arthropods.

We would add that, rejecting the old term Tracheata, the prosogoneate Myriopods appear to us to constitute an independent phylum, rather than a subphylum, and for that reason we have ventured to propose the name *Meropoda* for the group (μερος, a part or segment; πους, ποδος, a leg), from the fact that the mandibles are more distinctly divided into segments than in any other group of Arthropods, thus more closely resembling the other appendages of the body, whence it follows that all the limbs, including the mandibles, have the primitive feature of being composed of several segments.

The Symphyla in respect to the structure of the mandibles are less primitive than the Diplopods, but I am now inclined to agree with those who have pointed out their Diplopods affinities and to place them among the Meropods.

The Systematic Position of the Symphyla.—This is a puzzling problem. In my *Text-Book of Entomology* I have with some care reviewed the chief points in the anatomy of Scolopendrella and the opinions of different authors regarding its systematic relations. Having studied sections of the animals, I prepared a figure or reproduction from the sagittal sections of a female, of which the accompanying illustration (Fig. 1) is an enlarged reproduction.

Comparing the digestive tract with that of Pauropus, it is divided into three portions; the œsophageal valve opening into the stomach is seen at *œ. v.*, and the beginning of the rectum is well marked; the two urinary tubes are large, arising at the posterior end of the intestine and ending in front at the third segment from the head

[1] *Zoologische Anzeiger*, xvi, Jahrg. 3, Juli, 1893, p. 271.

[2] *Natural Science*, x, February, 1897, p. 114.

[3] Bronn's *Klassen. u. ord. Thier-reichs*, Bd. V, VI, Abth. Arthropoda, Leipzig, 1902.

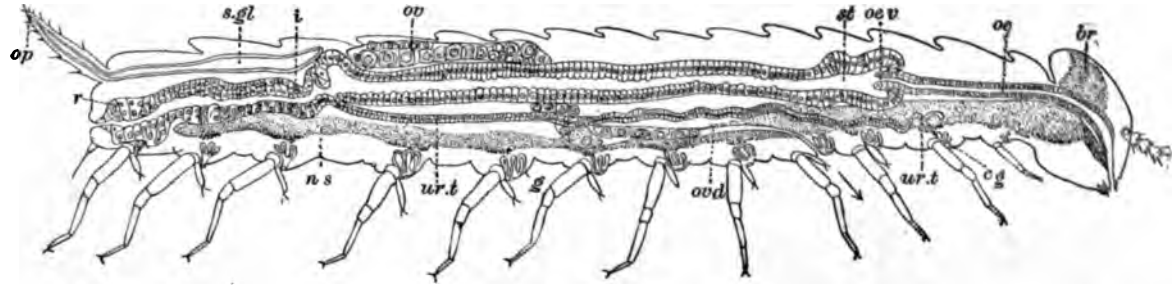


FIG. 1. Section through *Scolopendrella immaculata*; *œ*, œsophagus; *œ.v*, œsophageal valve entering the stomach; *i*, intestine; *r*, rectum; *br*, brain; *ns*, abdominal chain of ganglia; *ovd*, oviduct; *ov*, ovary; *s.gl*, silk gland, and *op*, its outer opening in the cercus; *ur. t*, urinary tube; *cg*, coxal glands or blood-gills.—Author, *del.*

(*ur. t.*). The ovary (*ov.*) is seen to lie partly beneath but mainly above the intestine; the median opening of the oviduct (*ovd.*) being indicated by the arrow between the third and fourth pair of legs. Attention should be called to the eversible coxal sacs (*c. g.*), of which there are eleven pairs situated at the base of the legs of each pair; the sac is largest and most developed in the middle of the body and is a convoluted tube which makes three turns. The silk gland (*s. gl.*) at the end of the body is large, its direct opening situated at the end of the cercus, while the gland itself extends as far forward as the third segment from the end of the body. The brain and nerve-cord are large and thick, much as in Pauropus. The dorsal vessel, fat body, rectal glands and the salivary glands are not represented.

There is in Scolopendrella a mixture of Diplopod and Thysanuran characters, the former the more primitive and predominating. My original idea that it is a Thysanuran is certainly a mistaken one. The Symphyla evidently forms a group by itself, and I am inclined to agree with Pocock and with Kingsley that it should for the present be associated with Pauropods and Diplopods. Yet were it not for the anterior position of the genital opening we should regard it as the representative of a group from which the insects have descended.

The Symphyla is evidently a much less primitive group than the Pauropoda and Diplopoda, as proved by the single genital opening and the Thysanuran characters it possesses. It would seem as if it had already begun to diverge from the Diplopod stem, and was becoming modified in the direction of the Thysanura.[1] It is a true composite or prophetic type which has persisted from very early paleozoic times, and we may well imagine that there once existed a form intermediate between it and the Thysanura in which the genital outlet had moved back to the position it holds in Chilopods and insects. As I state in my *Text-Book of Entomology*, "certainly Scolopendrella is the only extant Arthropod which, with the

[1] The thysanurous characters and the fact that it has but a single pair of legs to a segment (unless, as Schmidt suggests, the parapodia "represent the vestiges of a second pair of legs and correspond to the hinder pair of limbs of the primary double segment," thus indicating I would add the diplopod origin of Scolopendrella) appear to indicate that it is a form which has become considerably detached from the Diplopod stem, and has gone part way towards the incoming Thysanura. Campodea also possesses these so-called "parapodia."

sole exception of the anteriorly situated genital opening, fulfills the conditions required of an ancestor of Thysanura, and through them of the winged insects" (p. 22). Meanwhile, until the embryology of this form is thoroughly worked out and compared with that of the Diplopods on the one hand, and Campodea, as treated by Uzel, on the other, we must be content to let the Symphyla remain provisionally associated with the Diplopoda in the phylum Meropoda.

Phylum IV. PROTRACHEATA. Class *Malacopoda.* The arthropodan features of Peripatus are discussed in my *Text-Book of Entomology* (p. 9). Its nature as the probable ancestor of the Chilopoda is, notwithstanding the immense gap between it and Chilopods and insects, such as to still compel us to suppose that it resembles the probable progenitor of the Chilopods and of the insects. It would be difficult to know what better to do with it. It certainly cannot be placed among the Annelids, or in any other Arthropodan phylum, and it is with little doubt a very ancient type which has persisted from perhaps early paleozoic times.

Phylum V. ENTOMOPTERA. Class *Chilopoda* and class *Insecta* (Hexapoda). While the Chilopoda are the nearest allies of the Insects, there is certainly a wide gap between them, and there are no structures in insects which unmistakably point to their origin from Chilopods, although Uzel in his account of the embryology of Campodea shows that in some respects it develops like Geophilus. At present, however, we are in the dark as to the origin of the thysanurous Synaptera from any form, unless we invoke a Scolopendrella-like ancestor in which the genital opening has moved back to a position homologous with that of Peripatus, Chilopods and insects.

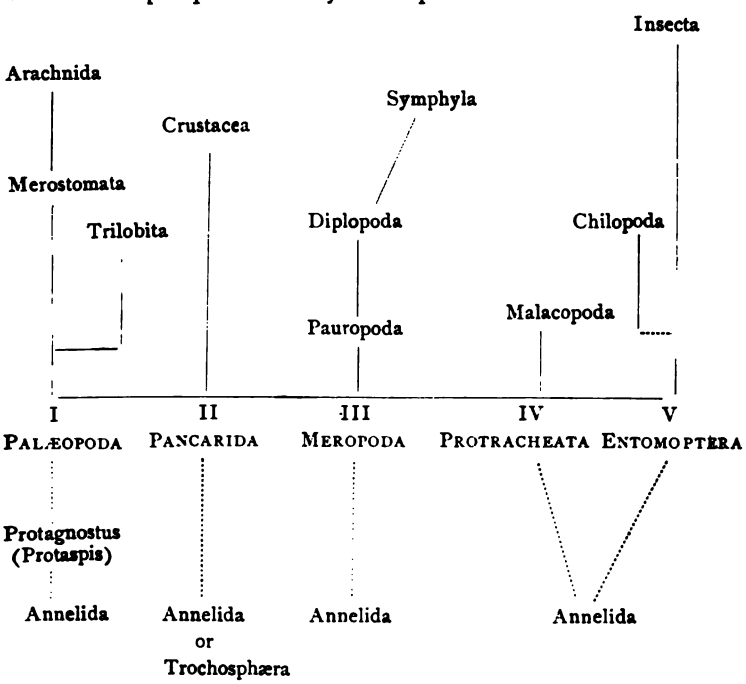
The combination of Chilopoda and Insecta as here given is a new one,[1] and for the Phylum, as we limit it, a new name seems necessary. As the Chilopods are a quite subordinate group, and the great mass of the orders is composed of winged forms, I have ventured to propose the term *Entomoptera* to cover this great group of Arthropodous animals, reserving the name Insecta for the class which has always borne that name. Each of the phyla as here limited appears, judging from their structure and what we know of their development, to represent distinct and independent lines of development, and are submitted for consideration by zoologists. It

[1] The *Antennata* of Lang comprises all the Myriopoda and the Insecta.

may at least be claimed that the breaking up of the Arthropoda into more definite, well-circumscribed groups will lead to greater exactitude and definiteness when referring to them.

After this article was completed I discovered that A. C. Oudemans as early as 1886 thus expressed his views as to the naturalness of the Arthropoda: "It is desirable that the group of Arthropoda should be given up. The groups of Acaroidea, Arachnoidea, Crustacea, Pantopoda, Onychophora, and Insecta are independent of each other, and should, therefore, be treated separately in the manuals. The very complicated structure would then become clearer to the student. A comparison of the groups with each can best take place afterwards and not beforehand" (*l. c.*, p. 20).

The following diagram will roughly indicate the different Phyla and the principal classes into which they are divided. It should be observed that the Annelidan ancestors of any of these five Phyla probably had few trunk-segments, being probably primitive Trochozoa with parapodia already developed.



PROVIDENCE, R. I., March 28, 1903.

THE PRINCIPLE OF LEAST WORK IN MECHANICS AND ITS USE IN INVESTIGATIONS REGARDING THE ETHER OF SPACE.

BY MANSFIELD MERRIMAN.

(*Read April 2, 1903.*)

The principle of least work has been extensively used in applied mechanics since 1879, when it was first formally stated and established by Castigliano. Previous to that time, various authors had discussed the principles of least action, of least constraint, and of least resistance, and had applied them in the solution of special problems. The principle of least work, however, is capable of more definite statement and demonstration than the other minimum laws, and its range of application in statical investigations on elastic structures is wide, while it has been found to be of great practical value to civil engineers.

When a structure like a bridge truss contains members sufficient to prevent distortion of its panels and no more, the stresses in these members due to given loads can be readily computed by the principles of rigid statics, the members in this case being called necessary ones. If there be superfluous members, however, rigid statics cannot determine the stresses, since the number of unknown stresses is greater than the number of statical conditions. In this case the structure is said to be statically indeterminate, and the principle of least work must be applied. This principle asserts that the stresses under consideration have such values that the potential stress energy stored in all the members of the structure shall be a minimum. If there be n stresses under consideration and m statical conditions, the remaining $n - m$ conditions are expressed by $n - m$ equations, which are deduced by equating to zero the derivatives of the expression for the total stored energy, these being the conditions that render this energy a minimum.

As a simple example the case of a rectangular table with four legs may be considered, it being required to find the stresses in these legs due to single load placed on the table in a given position. This is a statically indeterminate problem, since rigid statics furnishes but three conditions, and the solution cannot be made if the legs are rigid. The legs are, however, really elastic and each one is shortened in supporting the load, the stress in each leg multiplied by the amount of shortening being proportional to the stored

energy in it. The amount of shortening is, moreover, proportional to the stress, if the elastic limit of the material be not exceeded. Accordingly, the stress energy in the four legs due to the given load is proportional to the sum of the squares of the four stresses, and this sum is to be made a minimum. This condition, in connection with the three statical ones, enables the four stresses due to the load to be readily determined for any given position of that load, and that these stresses actually occur is easily verified by experiment.

A close analysis of the principle of least work as applied to any framed structure will show that its applicability and its validity depend upon the fact that the longitudinal deformation of any member is proportional to the stress upon it. This law of elasticity, commonly known as Hooke's law, is closely true for the materials used in engineering structures, provided the elastic limit be not exceeded. In all cases of the design of structures it is intended that this limit shall not be surpassed, and hence the principle of least work may be used with confidence and success in computations of stresses in statically indeterminate trusses.

It is sometimes asserted that the principle of least work is a statement of a general law of nature which is obeyed not only by materials under stress but by animate beings. While it may be true that men and animals endeavor to perform their tasks in the way most economical of effort, this analogy has no bearing upon the demonstration of the principle of least work. For this demonstration rests upon the theorem of virtual velocities, the formula for the stored stress energy being the integral of that of virtual velocities. On analyzing this proof it is seen that the integration is rendered possible by the fact that the deformation of each member is assumed to be proportional to the stress upon it. This assumption indeed is the same as that of the superposition of forces, for it supposes each stress to produce its effects independently of the existence of other stresses. The theorem of virtual velocities applies to all cases of equilibrium, but its integral form does not give the principle of least work unless Hooke's law of elasticity is fulfilled. This principle, therefore, is of limited application in mechanics, and it states no general law of nature.

In the method of least squares the conditions and rules for finding the most probable values of observed quantities are derived from the principle that the sum of the squares of the residual errors

shall be a minimum. In theoretical mechanics the condition for finding the centre of mass of a system of bodies may be expressed by saying that the moment of inertia of the system shall be a minimum. In the mechanics of elastic bodies the principle of least work is analogous to these, for the conditions which must be fulfilled are those found by making the stored energy of the system a minimum. In all these cases the algebraic conditions are expressed by linear equations while the laws from which they are derived are in quadratic form, and these laws are only true when each elementary error or particle produces its effects independently of others.

Solid beams and tubes, as well as framed trusses, are subject to the principle of least work, provided the materials of which they are made conforms to Hooke's law of elasticity. For instance, the thick hollow cylinder of a gun tube is stressed under the pressure arising from the explosion of the powder, and the stress at any point varies inversely as the square of the distance between that point and the centre of the tube. It is easy to show that this law of variation is the one which makes the stored energy in the tube a minimum. So in a hollow sphere subject to interior pressure, the stresses throughout the spherical annulas vary inversely as the cubes of their distances from the centre, and this law of variation is the one which renders the stored stress energy a minimum.

The ether that fills space and transmits the force of gravitation from every particle of matter to all others has been regarded by many physicists as an elastic solid which obeys Hooke's law. If so it must be subject to the principle of least work. Any portion of matter may be supposed to exert upon the ether a compressive force, due to the fact that its molecules have displaced the ether and crowded it outwards. Then the stresses in the ether due to this displacement must be so distributed that the stored energy in the infinite sphere may be a minimum. Stating the algebraic expression for this energy due to a spherical body, it is found that its minimum value occurs when the stress at any point in the ether varies inversely as the cube of the distance of that point to the centre of the body. If gravitation be a differential effect, due to the difference of the stresses upon opposite sides of a body, the force of attraction between two spheres should vary inversely as the fourth power of the distance between their centres. From no point of view does it seem possible to deduce the actual law of

gravitation from the stresses which must exist in the ether under the supposition of perfect elasticity.

The use of the principle of least work in investigations regarding the ether of space hence leads to negative results, as far as its applicability is concerned. It indicates, however, the important conclusion that the ether is not an elastic substance in which stresses are proportional to deformations, and accordingly studies concerning it should be based upon other suppositions concerning its properties. Since the ether cannot be made the object of experiment and since all we know concerning it is from rough analogy and indirect evidence, negative conclusions are valuable. By successively discussing and rejecting one assumption after another, it is possible that in due time properties of the ether may be found which will explain not only the inertia and gravitation of matter, but also the phenomena of electricity and magnetism.

LEHIGH UNIVERSITY, SOUTH BETHLEHEM, PA.

THE "FRANKLIN PAPERS" IN THE AMERICAN PHILOSOPHICAL SOCIETY.

BY J. G. ROSENGARTEN.

(*Read April 3, 1903.*)

In the collection of this Society there are some seventy large folio volumes of "Franklin Papers." Franklin left all his papers to his grandson, William Temple Franklin, who, after a long interval, published in London and Philadelphia six volumes of Franklin's works. Of course, this represented but a small part of his papers. Those used in the preparation of Temple Franklin's edition are now the property of the United States, which has never yet printed a Calendar of them. Temple Franklin selected from his grandfather's papers those that he thought suitable for publication, and left the rest in charge of his friend, Charles Fox, to whom he bequeathed them, and Charles Fox's heirs, in turn, after a long lapse of years, presented them to the American Philosophical Society, in whose custody they have remained ever since. They have been roughly classified, and are bound in a rude and careless way. Under the present efficient Librarian, Dr. Hays, a Calendar is being made as fast as the limited means at his disposal will per

mit, and when that is completed, it is hoped that it will be printed as a useful guide to the miscellaneous matter collected here. Sparks and Hale and Ford and Parton and Fisher and others who have written about Franklin have used them, but even the most industrious student may well be appalled at the labor required to master all the contents of these bulky volumes, representing Franklin's long and many-sided activity.

He kept copies of most of his own letters and the originals addressed to him, often endorsing on them the heads of his replies. These volumes contain papers from 1735 to 1790—the first forty-four volumes letters to him; the forty-fifth, copies of his own letters; the forty-sixth, his correspondence with his wife; the forty-seventh and forty-eighth, his own letters from 1710 to 1791; the forty-ninth, his scientific and political papers; the fiftieth, his other writings—notably his *Bagatelles*, those short essays which had such a vogue and are still read; the fifty-first, poetry and verse, his own and that of others, no doubt selected by him for use in his publications; the fifty-second, the Georgia papers—he was agent for that colony; and the remaining twenty volumes, all the multifarious correspondence, other than official, mostly during his long stay in France, his various public offices at home and abroad, his enormous correspondence about appointments from men of all nationalities, who wanted to come to America, under his patronage, to fight, to settle, to teach, to introduce their inventions, for every imaginable and unimaginable purpose.

Both in England and France he kept all notices of meetings, such as those of the Royal Society and other scientific bodies of which he was a member, invitations, visiting cards, notes, business cards, etc., and at home he kept copies of wills, deeds, powers of attorney, bonds, agreements, bills, etc., and drafts, checques, bills of lading, public accounts, and even certified copies of Acts of Congress, and account books, and, in addition, Temple Franklin left eight volumes of letters to him from 1775 to 1790.

In this mass of material his biographers have found much that was of value, but there remains almost untouched the interesting correspondence of his friends in England during the years before and those of the War of Independence. There are examples of his own clever *jeux d'esprit* in the "Intended Speech for the Opening of the Parliament in 1774," in which the king himself is made to foretell the "seven or ten years' job" that his "Ministers have

put upon him to undertake the reduction of the whole Continent of North America to unconditional submission." His friend Hartley sent it to him in 1786, when the prophecy had been fully realized. Again in 1778 he received a full report of the famous dying speech of Chatham, and of that of Lord Shelburne in his defense of the American cause.

During these eventful years, his correspondents in England and in the Colonies kept him well informed both of the actions and plans of the Government and of the Opposition. Some of these may be of interest as showing how earnestly both sides were presented to him that he might use his influence to maintain peace. Priestley, who was then the Secretary of Lord Shelburne, writes from London, in February, 1776, with a due report of political and scientific information, and Lee and Wayne write to him during the campaign which was to end in Burgoyne's surrender, and thus contribute largely to the alliance with France, which owed so much to Franklin's influence not only with the French Court and French statesmen, but with the philosophers and the people.

His correspondence in Paris is a perfect picture of the time. One day he gets an invitation to attend experiments in electricity from a correspondent, Brogniart, who reports the successful treatment of sick people by electric fluid, in 1778, and soon after the Curé of Damvillers asks him for a cure for dropsy for one of his parishioners. One writer submits a plan for eliminating poverty in the United States, and Turgot asks what method Franklin advises for burning smoke and thus diminishing the consumption of wood, which was steadily getting dearer. Then comes from London an offer to disclose a method of refining common salt and using it to cure and preserve flesh and fish, for the modest fee of 2000 guineas. Genet, afterwards so well known from his troublesome career as French Minister in this country, reports progress made in August, 1778, in translation of the *Pennsylvania Gazette* accounts of battles for the French papers, and the same mail brings a letter asking Franklin's approval of mechanical and mathematical problems, and for news of Fouquet, Master Gunpowder Maker at York, Pa. Brogniart invites him to witness new experiments in electricity, and soon after he is told of a plan of six or eight Germans, men of letters and prominent position, to go to America to found a college, where the instruction can be given in Latin until the teachers have mastered English. He receives poems and eulogies

in all languages, and offers to write histories of the new Republic, provided Franklin will furnish material, maps, etc. Then comes a request to look into an invention to reunite broken bones in all cases of fracture. The Palatinate Academy of Sciences, at Mannheim, sends its works dealing with electricity, etc., and urges establishing a German Scientific Society in Philadelphia.

A man and wife, with six children and six farm laborers, desire to settle in America, and ask Franklin to get Congress to give them land near Philadelphia, enough for the support of twenty persons, their connections. Franklin notes that his reply was that land was so cheap in Pennsylvania, that there was no need to apply to Congress.

Then came an offer to establish a Swiss clock and watch factory at Boston or Philadelphia. Even Franklin's patience was tried by a request to explain the right of America to assert its independence, for on this letter he endorsed "Impertinent."

The letters are a perfect picture of Franklin's busy social life in Paris, with politics, science, literature, war, privateering, all represented in his correspondence.

There are many letters from John Paul Jones about his naval exploits, and frequent appeals for help in securing the release of prisoners captured at sea, for help to return them and other Americans in distress to their homes. Dr. Price writes from London to know if it is true that Washington is grown unpopular, and that his army deserts in great numbers, and that the suffering in America is excessive. William Strahan reminds Franklin that in 1763 he spoke of America as England's strongest ally and of France as that perfidious nation. Vaughan sends to Chaumont (who reports it to Franklin) a message of greeting for their friend who always carried spectacles on his nose and kingdoms on his shoulders.

His correspondence came from England and from all parts of the Continent and from the West Indies in an unending stream.

A very curious letter is one from Richard Penn, dated London, October 20, 1778, which I think has never been printed:

"*Dear Sir*:—Nothing but necessity could have induced me to take the liberty of begging your attention for a few moments, from those various and important affairs with which you are entrusted, and which you have executed with so much reputation to yourself and advantage to your country; at the same time I am aware that the name subscribed will not at first sight bring you much in favour

of the writer. Nevertheless I have too high an opinion of your character to imagine that any misunderstanding which might formerly have subsisted between you and any part of my family, in which I myself could have had no share, will not at all prejudice you against me and in any degree withhold you from lending me your advice and perhaps assistance upon the present occasion. I flatter myself I have some slight ground to go upon in this case, which I own I am most willing to catch at.

"I am married to your late ward, the eldest Miss Masters, and have now living with me her younger sister, still under age, and, of course, in a manner claiming your patronage, as well as their mother, the widow of your late friend. From this connection it is well known that I possess a very considerable property in the city of Philadelphia and its environs, besides two or three valuable estates of my own in the Province of Pennsylvania, a whole undivided Proprietary of New Jersey; yet with all this property, I have not been able for more than two years past to procure one shilling from that country, nor have during that time so much as received a line from my friend and agent, Mr. Tench Francis, who it is probable has at this time a handsome sum of money belonging to me in his hands. The purse I brought with me to England is nearly exhausted, tho' it has been managed with the strictest economy. I have not yet tried, nor would I willingly at present, what American security would produce in this country.

"I should think myself infinitely obliged to you if you could point out to me in what manner I could procure either from America, or in any other way, a temporary subsistence. I have not a doubt but that in time matters will turn out much to the advantage of everybody concerned and connected with that country.

"Let me entreat you to favor me with an answer to this letter under cover to my Bankers, Messrs. Barclay, Bevan & Co., No. 56 Lombard street, in doing which you will lay a lasting obligation upon one of the many who revere your character and admire your abilities.

"Give me leave to subscribe myself, Dear Sir,

"Your very sincere friend,

"RICHD. PENN."

When it is remembered that the hostility of the Penns to Franklin was so strong that Governor John Penn declined to be Patron

of the American Philosophical Society because it had chosen Franklin for its President, and that Richard Penn had been Lieutenant Governor (as Deputy for that uncle and his brother) from 1771 to 1773, it must have been difficult for Franklin not to feel that such a letter from such a man was indeed a tribute to his position, achieved solely by his own efforts.

From this mass of correspondence, I have selected some letters showing the state of public opinion in New England in 1774, and from London in 1775, including a characteristic letter from Priestley and from Charles Lee and Wayne in the field. Much more might be printed to show how well Franklin kept in touch with all that was of interest during his long and busy career. It is well that this venerable Society, so largely the result of his labors, should be made the custodian of the papers that follow almost his daily thoughts, and it is to be hoped that the preparation and publication of a Calendar showing their contents may be completed at no distant day, certainly by the two hundredth anniversary of the birth of our founder, and thus perpetuate his memory.

Franklin's legacy to the Philosophical Society was ninety-one volumes of the *History of the Royal Academy of Sciences* at Paris, thus helping that collection of publications of scientific societies that make so valuable a portion of its Library.

THE ORBIT OF THE DOUBLE STAR Σ 518.

BY ERIC DOOLITTLE.

(*Read April 3, 1903.*)

INTRODUCTION.

It is well known to astronomers that many of the stars of the sky which to the naked eye appear to be but single stars are when viewed with the telescope seen to be made up of two or more stars very close together. About twenty thousand such double stars have been measured and catalogued, and the number is continually being added to through the discoveries of the great modern telescopes. There are scarcely fifty of these, however, of which a determination of the orbit is possible.

It was in the years 1802 and 1803 that the classic memoirs of Herschel appeared, in which it was shown for the first time that

the two stars of a binary system revolve in elliptic orbits about their common center of gravity. The first method for determining the orbit of the companion star about its primary was given by Savary in 1827, who applied his method to the binary ξ Ursæ Majoris. This was thus the first double star of which an orbit was computed.

In the method of Savary, the elements of the orbit were derived from the least possible number of measures which would theoretically determine them. It was thus but very poorly adapted to secure good results, since all double star measures are liable to errors which are very large in proportion to the quantities to be determined from them. The method was improved by Encke, and other methods were subsequently devised by Sir John Herschel, Villarceau, Thiele and others; but in all of these the development was from the point of view of the pure mathematician, rather than from that of the practical astronomer.

The astronomer who essays to compute the orbit of a double star will find that he has at hand a great mass of measures, which, having been made by observers of varying experience and with instruments of all degrees of perfection, are more or less discordant. Each one of these measures consists of a determination at a given time of the distance and direction of the companion star from its primary.

If now these measures be plotted, by taking a point on the paper to represent the principal star and laying off from this point each measured distance and direction to the companion star, a series of other points will be obtained which will represent to the eye the path which the companion has pursued about its primary. Were the measures free from error, the points which indicate the position of the companion would lie accurately upon the perimeter of an ellipse; but practically they are very far from doing so, especially if the double star is very close and difficult of measurement.

The ellipse which the companion appears to describe does not represent the true orbit of the body in space, since the true orbit is viewed more or less obliquely. It is evidently the projection of the true orbit on a plane tangent to the celestial sphere at the point at which the double star is situated. While the true orbit in space is an ellipse of which the principal star occupies the focus, the apparent or projected orbit, though also necessarily an ellipse, will not have its focus at the principal star. Nevertheless, Kepler's

second Law, which states that the areas swept over by the radius-vector are proportional to the corresponding times, will evidently be true, provided that in the apparent orbit these radii-vectores are drawn from the principal star instead of from the focus.

Having plotted the series of measures as above described, the first step in the determination of a double star orbit is to draw the apparent ellipse in such a manner that it shall represent them reasonably well; the various sectorial areas are then measured with a planimeter, or otherwise, and the trial ellipse changed in shape and position until finally, after several trials, the measured positions and the law of areas are both approximately satisfied.

To fix the shape of the true orbit and its position in space, and to predict the future motion, there must next be determined the following seven elements:

(1) *The Period, P.* This can be measured directly from the apparent ellipse, since, by Kepler's Law, any sectorial area is to that of the whole ellipse as the time occupied in the description of the area is to the Period.

(2) *The Time of Periastron Passage, T.* This is the date at which the companion passes the nearer vertex of the true ellipse. It can evidently be found from the apparent ellipse by an application of Kepler's Law.

(3) *The Eccentricity, e.* This, since it is a ratio, can be obtained from the apparent ellipse.

(4) *The Inclination, i,* of the true orbit to the tangent plane.

(5) *The Longitude, Ω,* of the intersection of two planes.

(6) *The Longitude, λ,* of periastron.

The last three elements are obtained by solving a spherical triangle. The longitudes are measured from the hour circle passing through the star, from the north point in the direction of motion.

(7.) *The Semi-Major Axis, a.*

The elements of the true orbit as thus obtained enable us to predict the direction and distance of the companion for any time. The next step of the computation is to obtain the computed distance and direction at the date of each observation. A comparison of the computed with the observed positions furnishes a basis for improving the elements by the principles of Least Squares. The same process is repeated with the improved elements, until a satisfactory agreement between the computed and observed positions is obtained.

The Computation.

There are available for this determination measures on 141 nights, as shown in the following table. In the first column will be found the date of observation; in the second, the measured distance; in the third, the measured angle; in the fourth, the number of nights on which the measures were made, and in the fifth, the name of the observer.

	Date.	ρ.	θ.	*n.*	*Observer.*
		" "	°		
1	1783.13	4 to 8	326.7	1	Herschel.
2	1825.12		287 ±	1	Struve.
3	1835 to '36	"			"
4	1850.94	3.96	156.60	2	Otto Struve.
5	1851.06	3 ±	159.96	1	Dawes.
6	1851.49	3.87	155.10	2	Otto Struve.
7	1853.64	3.93	158.30	3	"
8	1854.79	4.13	155.30	1	"
9	1856.80	4.51	152.90	1	"
10	1857.82	4.40	153.00	1	"
11	1864.84	4.45	147.60	2	Winnecke.
12	1865.89	4.26	143.95	2	Otto Struve.
13	1869.10	4.46	140.40	1	"
14	1871.99	2 ±	125 ±	1	Knott.
15	1872.56	4.62	140.65	1–2	Otto Struve.
16	1873.99	4.27	133.90	1	"
17	1874.10	4.39	135.70	1	"
18	1875.14	3.80	138.10	1	"
19	1876.11	4.01	130.50	1	"
20	1877.12	2 ±	120.	1	Flammarion.
21	1877.84	3.36	127.50	3	Cincinnati.
22	1877.84	3.92	128.24	6	Burnham.
23	1877.95	3.94	126.45	4	Dembowski.
24	1878.14	4.36	125.50	1	Otto Struve.
25	1879.05	3.49	125.38	4	Burnham.
26	1879.18	3.52	125.00	2	Hall.
27	1879.75	3.29	120.00	1	Cincinnati.
28	1880.09	3.28	121.30	5	Burnham.
29	1880.95	3.16	122.06	5	"
30	1881.84	3.53	119.00	6	"
31	1882.12	3.25	118.15	2	Hall.
32	1883.00	3.07	119.20	2	Burnham.
33	1883.81	3.10	115.80	2	Hall.
34	1884.16	3.74	118.20	1	Herman Struve.
35	1886.00	3.22	112.15	2	Leavenworth.

	Date.	*ρ.*	*θ.*	*n.*	*Observer.*
		"	°		
36	1886.09	3.00	112.23	6	Hall.
37	1886.92	3.01	111.03	3	Tarrant.
38	1887.14	2.56	109.18	1–4	Schiaparelli.
39	1888.08	2.26	109.48	2	"
40	1888.12	3.04	107.68	5	Hall.
41	1888.84	2.94	106.83	3	Burnham.
42	1888.87	2.81	105.05	3	Tarrant.
43	1889.03	2.87	107.59	1–2	Schiaparelli.
44	1889.12	2.79	103.55	4	Hall.
45	1890.73	2.68	99.95	4	Burnham.
46	1890.98	1.72	99.00	3	Hough.
47	1891.01	2.62	101.49	2	Schiaparelli.
48	1891.06	2.65	98.56	5	Hall.
49	1891.78	2.48	97.38	4	Burnham.
50	1893.21	2.18	93.8	1	Comstock.
51	1895.89		83.65	0–1	Doberck.
52	1895.91	2.32	87.4	1	Collins.
53	1897.97	2.62	77.22	3	Aitken.
54	1899.11	2.39	73.6	2	"
55	1899.80	2.30	68.35	3	Doolittle.
56	1900.92	2.40	63.41	2	"
57	1903.14	2.34	55.22	4	"

NOTES—(1) Herschel placed the pair in his "Class II," which indicates that he estimated the distance as between 4″ and 8″. Otto Struve considers that at this time the distance must have been less than 4″, which seems the more probable. No use has been made of this measure in the final adjustment. (2) Excessively difficult. The angle was estimated roughly as being in the direction of the principal star, of which the position angle is 107°. The entire unreliability of this measure was first pointed out by Burnham in 1894. (3) No trace of duplicity. (14) This is merely a rough estimate. Knott used a 7⅓ inch refractor. (51). "Nearly invisible." (53) and (54) Made with the 12-inch. I have given half the theoretical weight to numbers (5), (38) and (43). (57) Was not used in the computation; these observations were made after the work was completed.

These observations were corrected for precession, and then plotted as above described, and the elements of the true orbit were derived from them. These elements were the following:

$P = 180.084$ years
$T = 1842.72$
$e = 0.129$
$i = 61.^{\circ}78$
$\Omega = 148.^{\circ}76$
$\lambda = 321.^{\circ}22$
$a = 4.''681$

Elements of the First Approximation.

For the purpose of effecting a least square solution, twelve mal places were next formed from the observations of the preling table, as follows:

Date.	θ.	$\theta +$ Precession.	ρ.	*n.*
	$^{\circ}$	$^{\circ}$	$''$	
1852.48	156.89	157.13	3.95	9–8
1857.31	152.95	153.17	4.45	2
1867.95	143.54	143.70	4.42	7–6
1874.83	134.55	134.67	4.12	4
1879.03	124.89	124.99	3.56	31
1882.54	118.35	118.44	3.37	13
1887.71	108.53	108.59	2.93	31
1891.13	99.01	99.05	2.46	18
1894.06	90.60	90.62	2.25	2
1898.54	75.81	75.82	2.50	5
1899.80	68.35	68.35	2.30	3
1900.92	63.41	63.41	2.40	2

From these there resulted twelve equations between the six nown quantities, the residuals in angle only being employed. ese equations were weighted and solved for the corrections to elements, the results being as follows:

$P = 180.039$ years.
$T = 1843.122$
$e = 0.133$
$i = 62.^{\circ}96$
$\Omega = 150.^{\circ}01$
$\lambda = 320.^{\circ}24$
$a = 4.''681$

Elements of the Second Approximation.

The residuals from these elements were not wholly satisfactory, especially between the years 1853 and 1879, when they steadily maintained the positive sign. For the purposes of a further improvement, therefore, the original observations were next grouped into the following thirty-three normal places:

Date.	ρ.	θ.	*n.*	*Observer.*
	″	°		
1850.94	3.96	156.6	2	O. S.
1851.28	3.58	156.7	3	O. S., Da.
1853.64	3.93	158.3	3	O. S.
1854.79	4.13	155.3	1	"
1856.80	4.51	152.9	1	"
1857.82	4.40	153.0	1	"
1864.84	4.45	147.6	2	Wi.,
1865.89	4.26	144.0	2	O. S.
1869.10	4.46	140.4	1	"
1871.99	2 ±	125±	1	Kn.
1872.56	4.62	140.6	1–2	O. S.
1873.99	4.27	133.9	1	"
1874.10	4.39	135.7	1	"
1875.14	3.80	138.1	1	"
1876.11	4.01	130.5	1	"
1877.86	3.80	127.5	11	D., C. O., B.
1878.14	4.36	125.5	1	O. S.
1879.09	3.50	125.3	6	B., Ha.
1880.52	3.22	121.6	10	B.
1881.84	3.53	119.0	6	B.
1882.12	3.25	118.2	2	Ha.
1883.40	3.09	117.5	4	B., Ha.
1884.16	3.74	118.2	1	H. S.
1886.30	3.04	111.9	11	L., Ha., T.
1888.51	2.95	106.7	11	Ha., B., T.
1889.12	2.79	103.6	4	Ha.
1890.84	2.27	99.6	7	B., Ho.
1891.38	2.57	98.1	9	B., Ha.
1893.21	2.18	93.8	1	Com.
1895.90	2.32	85.5	1–2	D., C.
1897.97	2 62	77.2	3	A.
1899.53	2.34	70.4	5	A., Doo.
1900.92	2.40	63.4	2	Doo.

These measures were corrected for precession, and to the resulting 33 equations there were assigned two series of weights, the first depending only on the number of nights, and the second being

arbitrarily assigned. Only the residuals in angle were employed, so that there resulted 33 equations between the six unknowns. The final values obtained from this solution led to the following elements:

$$\left.\begin{aligned} P &= 180.0288 \pm 2.776 \text{ years.} \\ T &= 1843.185 \pm 1.051 \\ e &= 0.13423 \pm 0.0221 \\ i &= 63^\circ.25 \pm 0^\circ.74 \\ \Omega &= 150^\circ.82 \pm 0^\circ.71 \\ \lambda &= 319^\circ.54 \pm 0^\circ.57 \\ a &= 4''.791 \end{aligned}\right\} \text{ The Final Elements.}$$

The value of a was obtained from a series of equations of the form

$$a = \cos(\Omega - \theta c) \sec(v + \lambda) \frac{fo}{(\iota - e \cos \epsilon)} \ldots\ldots$$

The weighted mean was taken for the value of a.

The following table shows the agreement of the observed positions with the positions computed from the final elements. These residuals are perhaps as small as can be expected with a star of this character:

Date.	*θc.*	*Prec.*	*θc—pr.*	*θo.*	*θo—θc.*	*ρc.*	*ρo.*	*ρo—ρc.*	*n.*	*Observer.*
	°	°	°	°	°	″	″ to ″			
1783.13	327.33	+ 0.58	326.75	326.7	± 0.00	5.14	4 to 8		1	H.
1825.12	234.22	0.37	233.85	287 ±		1.95			1	S.[1]
1835.86	189.97	0.32	189.65			2.61			1	S.[2]
1850.94	160.15	0.25	159.90	156.60	— 3.30	3.98	3.96	"	2	O. S.
1851.06	160.00	0.25	159.75	159.96	+ 0.21	3.99	3 ±	— 0.02	1	Dawes
1851.49	159.45	0.24	159.21	155.10	— 4.11	4.01	3.87	— 0.14	2	O. S.
1853.64	156.83	0.23	156.60	158.30	+ 1.70	4.09	3.93	— 0.16	3	O. S.
1854.79	155.47	0.23	155.24	155.30	+ 0.06	4.15	4.13	— 0.02	1	O. S.
1856.80	153.10	0.21	152.89	152.90	+ 0.01	4.22	4.51	+ 0.29	1	O. S.
1857.82	151.91	0.21	151.70	153.00	+ 1.30	4.22	4.40	+ 0.18	1	O. S.
1864.84	144.06	0.18	143.88	147.60	+ 3.72	4.26	4.45	+ 0.19	2	Winnecke
1865.89	142.83	0.17	142.68	143.95	+ 1.27	4.23	4.26	+ 0 03	2	O. S.
1869.10	139.06	+ 0.15	138.91	140.40	+ 1.49	4.12	4.46	+ 0.32	1	O. S.
1871.99	135.49	0.14	135.35	125 +	—10.35	3.99	2 ±	— 1.99	1	Knott[3]
1872.56	134.80	0.14	134.66	140.65	+ 5.99	3.97	4.62	+ .65	1–2	O. S.
1873.99	132.89	0.13	132.76	133.90	+ 1.14	3.89	4.27	+ .38	1	O. S.
1874.10	132.74	0.13	132.61	135.70	+ 3.09	3.88	4.39	+ .51	1	O. S.
1875.14	131.31	0.12	131.19	138.10	+ 6.91	3.82	3.80	— .02	1	O. S.
1876.11	129.89	0.12	129 77	130.50	+ 0.73	3.75	4.01	+ .26	1	O. S.
1877.12	128.41	0.11	128.30	120 +	— 8 30	3.69	2 ±	— 1.69	1	Flammarion
1877.84	127.36	0.11	127.25	127.5	+ 0.2	3.64	3.36	— .28	3	C. O.
1877.84	127.36	0.11	127.25	128.24	+ 1.01	3.64	3.92	+ .28	6	B.
1877.95	127.19	0.11	127.08	126.45	— 0.63	3.63	3 94	+ .31	4	D.
1878.14	126.89	0.11	126.78	125.50	— 1.28	3.62	4.36	+ .74	1	O. S.
1879.05	125.44	0.10	125.34	125.38	+ 0.04	3.56	3.49	— .07	4	B.
1879.18	125.22	0.10	125.12	125.00	— 0.12	3.55	3.52	— .03	2	Hall
1879.75	124.25	0.10	124.15	120.00	— 4.15	3.51	3.29	— .22	1	C. O.
1880.09	123.67	0.10	123.57	121.30	— 2.27	3 48	3.28	— .20	5	B.
1880.95	122.19	0.09	122.10	122.06	— 0.04	3.42	3.16	— .26	5	B.
1881.84	120.67	0.09	120.58	119.00	— 1.58	3.35	3.53	+ .18	6	B.
1882.12	120.15	0.09	120.06	118 15	— 1 91	3.33	3.25	— .08	2	Hall
1883.00	118.48	0.08	118.40	119.20	+ 0.80	3.27	3.07	— .20	2	B.
1883.81	116.89	0.08	116.81	115.80	— 1.01	3.21	3.10	— .11	2	Hall
1884.16	116.21	0.08	116.13	118 20	+ 2.07	3.18	3.74	+ .56	1	H. Struve
1886.00	112.13	0.07	112.06	112.15	+ 0.09	3.04	3.22	+ 0.18	2	Leavenw'th
1886.09	111.94	0.07	111.87	112.23	+ 0.36	3 03	3.00	— 0.03	6	Hall
1886.92	110.10	0.07	110.03	111 03	+ 1.00	2.97	3.01	+ 0.04	3	Tarrant
1887.14	109.61	0.06	109.55	109.18	— 0.37	2.95	2.56	— 0.39	4–1	Schiaparelli
1888.08	107.43	0.06	107.37	109.48	+ 2.11	2.88	2.26	— 0.62	2	Schiaparelli
1888.12	107.32	0.06	107.26	107.68	+ 0.42	2.88	3.04	+ 0.16	5	Hall
1888.84	105.52	0.06	105.46	106 83	+ 1.37	2.82	2.94	+ 0.12	3	B.
1888.87	105.45	0.06	105.39	105.05	— 0.34	2.82	2.81	— 0.01	3	Tarrant
1889.03	105.05	0.05	105.00	107.59	+ 2.59	2.81	2.87	+ 0 06	2–1	Schiaparelli
1889.12	104.82	0.05	104.77	103.55	— 1.22	2.81	2.79	— 0.02	4	Hall
1890.73	100.39	0.05	100.34	99.95	— 1.39	2.70	2.68	— 0.02	4	B.
1890.98	99.70	0.05	99.65	99.00	— 0.65	2.68	1.72	— 0.96	3	Hough
1891.01	99.62	0.04	99.58	101.40	+ 1.91	2.68	2.62	— 0.06	2	Schiaparelli
1891.06	99.49	0.04	99.45	98 56	— 0.89	2.67	2.65	— 0.02	5	Hall
1891.78	97.40	0.04	99.36	97.38	— 1.98	2 63	2.48	— 0.05	4	B.
1893.21	93.10	0.04	93.06	93.8	+ 0.70	2.55	2.18	— 0.37	1	Comstock
1895.89	84.17	0.02	84.15	83.65	— 0.50	2.43			1	Doberck[4]
1895.91	84.14	0.02	84.12	87.4	+ 3.30	2.43	2.32	— 0.11	1	Collins
1897.97	76.73	0.01	76.72	77.22	+ 0.50	2.36	2.62	+ 0.26	3	Aitkin[5]
1899.11	72.49	0.00	72.49	73.6	+ 1.10	2.33	2.39	+ 0.06	2	Aitkin
1899.80	69.50	0.00	69.50	68.35	— 1.15	2.32	2.30	— 0.02	3	Doolittle
1900.92	65.61	0.00	65.61	63.41	— 2.20	2.31	2.40	+ 0.09	2	Doolittle
1903.14	57.19	0 .02	57.21	55.22	— 1.99	2.34	2.46	+ 0.12	4	Doolittle

[1] "A very vague estimate" (O. S.).
[2] "No trace of duplicity."
[3] This is a rough estimate merely; Knott used a 7½-inch.
[4] "Nearly invisible."
[5] 12-inch.

There have been two determinations made of the parallax of this star; the first determination was by the heliometer by Gill in 1882, and the second was by micrometric measures by Hall in 1884. The results were:

Gill,	o″.16	19.6 light years.
Hall,	o″.22	14.6 light years.

If we assume the mean of these, or o″.19, as the most probable value, the dimensions of the orbit and the combined mass of the two components can readily be determined. We find that the sum of the masses of the two components is nine-tenths the mass of our sun, and that the semi-major axis of the true orbit is 23.5 times the distance from the earth to the sun. The orbit is thus larger than the orbit of Uranus, but inferior to that of Neptune.

UNIVERSITY OF PENNSYLVANIA, PHILADELPHIA.

SOME ABORIGINAL LANGUAGES OF QUEENSLAND AND VICTORIA.

BY R. H. MATHEWS, L.S.,

MEMB. ASSOC. ETRAN. SOC. D'ANTHROP. DE PARIS.

(Read October 3, 1902.)

Last year I contributed to this Society a short description of the Gundungurra, one of the native tongues of New South Wales. In the following pages it is proposed to furnish the outlines of the grammatical structure of some aboriginal languages spoken by the native tribes of Queensland and Victoria.

The method of spelling adopted is that recommended by the Royal Geographical Society of London, with the following qualifications:

As far as possible vowels are unmarked, but in some instances the long sound of a, e, and u are indicated thus, ā, ē, ū. In a few cases, to avoid ambiguity of pronunciation, the short sound of u is thus represented, ŭ.

G is hard in all cases. R has a rough, trilled sound, as in "hurrah!" W always commences a syllable or word. Y at the beginning of a word or syllable has its ordinary consonant value.

The sound of the Spanish ñ often occurs; at the beginning of a

word or syllable I have given it as *ny*, but when terminating a word the Spanish letter is employed.

Ng at the beginning of a word or syllable has a peculiar nasal sound. At the end of a syllable it has substantially the sound of ng in "sing."

Dh is pronounced nearly as th in "that," with a slight sound of d preceding it. Nh has likewise nearly the sound of th in "that," but with an initial sound of the n. A final h is guttural, resembling ch in the German word "bach."

T is interchangeable with d, p with b, and g with k, in most words where these letters are used.

Ty and dy at the commencement of a word or syllable has nearly the sound of j. At the end of a word ty or dy is pronounced nearly as tch in "batch" or "ditch," omitting the final hissing sound.

All the details supplied in this article were taken down by myself from the lips of the natives speaking the languages herein dealt with—a tedious and laborious task.

The Murawarri Language.

In a communication to this Society in 1898 I described the social divisions and laws of intermarriage prevailing in the Murawarri tribe, together with a comprehensive list of totems, and will now proceed to exhibit the structure of their language. This tribe occupies an extensive region on the southern frontier of Queensland, between the Warrego and Culgoa rivers, reaching also some distance into New South Wales. Languages similar in grammar to the Murawarri, although differing somewhat in vocabulary, extend northerly into Queensland for hundreds of miles.

NOUNS.

Number.—Nouns have three numbers, the singular, dual and plural. Gula, a kangaroo. Gulabural, a pair of kangaroos. Guladhunna, several kangaroos. The suffix dhunna is frequently shortened to dhu, in rapid conversation.

Gender.—Mēn, a man. Mugiñ, a woman. Guthera, a small boy. Gutheragamba, a small girl. The sex of animals is distinguished by using, after the name of the creature, the words dhungur, male, and guni, female, and these words take inflexion for number and case.

Case.—The principal cases are the nominative, causative (or nominative-agent), genitive, accusative, instrumental, dative and ablative.

The nominative merely names the animal or thing, as, nguruñi, emu; dhagguñi, padamelon; wirri, bandicoot; wagan, crow; mulli, boomerang; kinni, yamstick; gūndal, dog; gugai, opossum; ngura, a camp; wungga, a bird's nest.

Causative: Guladyu ngunna wirrunga, a kangaroo me scratched.

Instrumental: Mēndyu wagan mullinyu bundhara, a man a crow with a boomerang hit.

Genitive: Mugingu kinni, a woman's yamstick. Wagangu wungga, a crow's nest.

Dative: Dhan yanna nguranggu, come to the camp.

Ablative: Dhirri yanna ngurango, go away from the camp.

Accusative: This is the same as the nominative.

ADJECTIVES.

Adjectives are placed after the nouns they qualify, and are similarly inflected for number and case.

Nominative: Gundal kittyu, a dog small; gundalbural kittyubural, a couple of small dogs; gundaldhu kittyudhunna, several small dogs.

Causative: Mugindyu thurdadyu guthera bundhara, a woman large a child beat.

Genitive: Mēngu thurdagu mulli, the large man's boomerang.

Adjectives are compared by using such phrases as, thurda nhu, kittyu nūngga, large this, small that. Superiority is implied by saying, thurdaburra, very large.

PRONOUNS.

Pronouns are inflected for number and person, and comprise the nominative, possessive and objective cases, some examples of which are given in the following table. There are forms in the first person of the dual and plural to express the inclusion or exclusion of the party addressed:

Singular.

	Nominative.	*Possessive.*	*Objective.*
1st Person.....	Ngadhu	Ngundi	Ngunna
2d "	Ngindu	Ingga	Bunga
3d "	Yallunggo	Ngumboga	Bunha

Dual.

1st Person	.....Ngulli	Ngulliga	Ngullinya
	.....Ngulliñūmba	Ngulligilunna	Ngullinyanumba
2d "	.....Nula	Nulaga	Nulanna
3d "	.....Yallabural	Bulaga	Burannha

Plural.

1st Person	.....Nginna	Nginnaga	Ngurranna
	.....Nginnadyula	Nginnagadyula	Ngurranadyula
2d "	.....Nura	Nuraga	Nuranna
3d "	.....Yalladhunna	Dhurraga	Dhurrana

There are forms of the pronouns signifying "to me," "from me," "with me," and so on, as in the following few illustrations:

Dhangandhera dhiga, he brought it to me.

Dhirrithunggia dhigamil, he ran away from me.

Ngunnhura niambu, with me rests he.

Interrogatives: Ngannga, who? Nganngabural, who (dual)? Nganngadhunna, who (plural)? Ngangagu, whom belonging to? Minya, what? Minyanggu, what for?

Demonstratives: This, nhu; that, nhurana. These demonstratives are very numerous, according as the object referred to is in front of, behind, near, or far from the speaker. Many of them take inflexion for number and person.

VERBS.

Verbs have the singular, dual and plural numbers, the usual persons and tenses, and three principal moods—indicative, imperative and conditional. There is a distinctive form of the verb for each tense—present, past and future; but number and person are shown by short pronominal suffixes to the stem of the verb. These rules will be readily understood on perusing the following conjugation of the verb, bundhera, to beat:

Indicative Mood—Present Tense.

Singular....	1st Person.....I beat		Bundhiyu
	2d " .....Thou beatest		Bundhindu
	3d " .....He beats		Bundhibu
Dual.....	1st Person.....	We, incl., beat	Bundhili
		We, excl., beat	Bundhilinūmb
	2d "	You beat	Bundhinula
	3d "	They beat	Bundhibula

Plural.....	1st Person.....	We, incl., beat	Bundhina
		We, excl., beat	Bundhinadyula
	2d "	You beat	Bundhinura
	3d "	They beat	Bundhira

Past Tense.

Singular....	1st Person.....	I beat	Bundharanyu
	2d "	Thou beatest	Bundharandu
	3d "	He beat	Bundharabu

Future Tense.

Singular....	1st Person.....	I will beat	Būnggunyu
	2d "	Thou wilt beat	Būnggundu
	3d "	He will beat	Būnggubu

It is thought unnecessary to give the dual and plural numbers of the past and future tenses.

Imperative Mood.

Positive.....Beat	Būngga
Negative.....Beat not	Būngga wulla

Conditional Mood.

I may beat	Wullawurri būnggunyu

Reflexive.

I am beating myself	Bundherriyu
I was beating myself	Bundherriaiyu
I will beat myself	Bundherriguyu

The inflexion continues through all the persons.

Reciprocal.

Dual.....	We, incl., are beating each other	Bumbullāli
	We, incl., will beat each other	Bumbullaguli
Plural.....	We, incl., are beating each other	Bumbullana
	We, incl., will beat each other	Bumbullaguna

The second and third persons of the dual and plural also take reciprocal inflexion.

The following examples show the native way of expressing the English verb "to be":

PROC. AMER. PHILOS. SOC. XLII. 173. M. PRINTED AUG. 1, 1903.

Present.....I am well	Murriñ indiyu (well am I)
Past .....I was well	Murriñ indayu (well was I)
Future .....I will be well	Murriñ inguyu (well will be I)

ADVERBS.

Yes, kaila. No, wulla. Here, nunggo. There, ngurra. Now, kunyegaila. By and bye, kunye. Yesterday, gūnda. Tomorrow, būrda. A few days ago, buggera dhurungga. Long ago, muttyagaila. Perhaps, wullawurri. Slowly, mūn-gi. Rapidly, kurdugurdu. Where, dhirrungga? Where (if two), dhirrambula? Where (plural), dhirradhunna? How many, minyungurra?

PREPOSITIONS.

In front, kurbu. Behind, billungga. In the rear, durungga. Inside, mugungga. Outside, bullungga. Beside me, gurgungga dhiga. Between, dhunnūngga. Down, burrungga. Up, gūnda. Over or across (referring to a river, hill, etc.), gurrundha. This side of, nhubarañ. The other side, beyond, gowurrigurrundha. Through, gaimyu. Towards, dhai. Away from, dhirra.

Several prepositions take inflexion for number and person: Behind me, billunggadhiga. Behind thee, billunggabunga. Behind him, billunggabuga. Behind us, billunggangurriga, and so on.

CONJUNCTIONS AND INTERJECTIONS.

It is not thought necessary to supply illustrations of these parts of speech.

NUMERALS.

One, yaman. Two, kubbo. Several, murabirri.

The Wamba Wamba Language.

This language is spoken among the remnants of the native tribes about Swan Hill on the Murray river, and extending southerly into the State of Victoria beyond Lalbert and Tyrrell creeks, the lower Avoca river, etc. The people are divided into two phratries, Gamaty and Gurgity, the men of one phratry marrying the women of the other. For lists of totems attached to these phratries, the reader is referred to a paper I contributed in 1898 to the Anthropological Society at Washington.[1]

[1] "The Victorian Aborigines: their Initiation Ceremonies and Divisional Systems," *American Anthropologist*, Vol. xi, pp. 333, 334. Map of Victoria, Plate V.

All the languages spoken in the eastern portion of Victoria are identical in grammatical structure with the Gundungurra language reported by me to this Society last year, although their vocabularies are altogether different. Westward of the 145th meridian of longitude all the Victorian languages have the same structure as the Wamba Wamba, with the exception of a strip of country on the lower Murray river.

NOUNS.

Number.—Karrange, a kangaroo. Karrange bullang, two kangaroos. Karrange girtāwal, several kangaroos.

Gender.—Wurtūnge, a man. Laiur, a woman. Banggo, a boy. Bannulaiur, a girl. Bupu, a child of either sex. The sex of animals is indicated by using the word mamo for males, and baba for females; thus, willunge mamo, a male opossum; willunge baba, a female opossum.

Case.—The nominative: Wanne, a boomerang. Kenninge, a yamstick. Wirrangin, a dog. Lūrnge, a camp.

The Causative: Wurtulu karange dhakkin, a man hit a kangaroo. Laiuru bupu dhakkin, a woman beat a child.

Possessive: Wurtua wanne, a man's boomerang. Every object over which ownership can be exercised is subject to inflection for number and person, thus:

Singular	1st Person.....My boomerang		Wannai
	2d " .....Thy boomerang		Wannin
	3d " .....His boomerang		Wannu

This declension extends to all the persons and numbers, in each of which one example will be sufficient:

Dual.....Our, inclusive, boomerang	Wannul
Trial.....Our, inclusive, boomerang	Wannangurkullik
Plural.....Our, inclusive, boomerang	Wannungur

Dative: Lūrndal, to the camp.

Ablative: Lūrnung, from the camp.

ADJECTIVES.

Adjectives follow the noun qualified, as kūrwinge kurong-untu, an emu large. Kurwinge bannutu, an emu small. They are inflected for number and case like the nouns, and comparison is effected as in the Murawarri

PRONOUNS.

Pronouns have four numbers, singular, dual, trial, and plural. There are double forms of the first person to include or exclude the person spoken to. The following table shows the nominative and possessive pronouns:

Singular.

1st Person.....I	Yetti	Mine	Yenneu
2d " .....Thou	Nginma	Thine	Nginneu
3d " .....He	Kinyi or Kalu	His	Kikinga

Dual.

1st Person... We, incl.,	Ngulli	Ours, incl.,	Ngullidha
1st Person... We, excl.,	Ngullu	Ours, excl.,	Ngulludhu
2d " You	Nyula	Yours	Nyuladhu
3d " They	Kalubulang	Theirs	Kinyebuladhu

Trial.

1st Person... We, incl.,	Yangurkullik	Ours, incl.,	Yanguradhukullik
1st Person... We, excl.,	Yandakkullik	Ours, excl.,	Yandhadhukullik
2d " You	Ngutakullik	Yours	Ngutadhukullik
3d " They	Kaludhanakullik	Theirs	Dhanadhukullik

Plural.

1st Person... We, incl.,	Yangur	Ours, incl.,	Yanguradhu
1st Person... We, excl.,	Yandhank	Ours, excl.,	Yandhadhu
2d " You	Nguta	Yours	Ngutadhu
3d " They	Kaludhana	Theirs	Dhanadhu

There are objective forms of the pronouns, signifying me, with me, towards me, from me, and so on. Interrogative and demonstrative pronouns are also various and precise.

VERBS.

Verbs have the same numbers and persons as the pronouns, three tenses and three principal moods; as exhibited in the following conjugation of the verb "to sit":

Indicative Mood—Present Tense.

Singular .... 1st Person.....I sit		Ngangan
Singular .... 2d " .....Thou sittest		Ngangar
Singular .... 3d " .....He sits		Nganga

Dual.....	1st Person...	We, incl., sit	Ngangangul
		We, exclu., sit	Ngangangullu
	2d "	You sit	Nganganyulu
	3d "	They sit	Ngangabulang
Trial.....	1st Person...	We, incl., sit	Ngangangurkullik
		We, excl., sit	Ngangandhankullik
	2d "	You sit	Ngangangutakullik
	3d "	They sit	Ngangandhanakullik
Plural.....	1st Person...	We, incl., sit	Ngangangur
		We, excl., sit	Ngangandhak
	2d "	You sit	Nganganguta
	3d "	They sit	Ngangandhana

Past Tense.

Singular	1st Person.....	I sat	Nganginan
	2d "	Thou sattest	Nganginar
	3d "	He sat	Ngangin

Future Tense.

Singular	1st Person.....	I will sit	Nganginyan
	2d "	Thou wilt sit	Ngangiuyar
	3d "	He will sit	Ngangiñ

The remaining moods are omitted, being similar in constitution to those of the Murawarri.

This is the first occasion on which the *trial*, or *triple*, number has been reported in the verbs of any Australian language. Mr. J. J. Carey, from the MS. of the late Mr. F. Tuckfield,[1] published a list of pronouns in what he calls the Woddowro language, but which I spell Wuddyâwurru, in which he shows an incomplete set of trial pronouns. He did not, however, observe the double form in the first person of the dual, trial and plural, which is now communicated by me in the languages of Victoria for the first time.[2]

Among the native tribes on the upper Campaspe, Lodden and Avoca rivers, instead of *kullik* being the sign of the trial, the word *baiap* is employed, as, Ngurnabuingunyinbaiap, we three sit.

[1] *Rep. Aust. Assoc. Adv. Sci.*, Vol. vii, p. 842 and p. 853.

[2] I have, however, previously discovered and reported the existence of two forms of the first person of the dual and plural in the nouns, pronouns, verbs, adverbs and prepositions of the Gundungurra, one of the native languages of New South Wales: PROC. AMER. PHILOS. SOC., Vol. xl, pp. 140–148.

Tyilbuingunyinbaiap, we three beat. It will be apparent that the words *baiap* or *kullik* are merely superadded to the suffix of the plural.

In the Motu, one of the languages of New Guinea, Rev. W. G. Lawes reports that the dual and trial of pronouns are formed by additions to the plural.[1]

If a line be assumed to be drawn on the map of Victoria from Melbourne to Echuca, then the whole of that portion of Victoria situated on the eastern side of that line has no trial number in its speech, but in all the languages to the west of that line the trial number obtains.

ADVERBS AND PREPOSITIONS.

In principle these resemble the same parts of speech in the Murawarri and Gundungurra, and some of them take similar inflexion for number and person.

Interjections and exclamations are not numerous and have been omitted.

NUMERALS.

One, yuwaia. Two, bulle. Several, girtāwal.

A NEW FRESH-WATER MOLLUSCAN FAUNULE FROM THE CRETACEOUS OF MONTANA.

(Plate IV)

BY TIMOTHY W. STANTON.

(*Read April 3, 1903.*)

An interesting collection of fresh-water invertebrate fossils, collected in Montana by a recent expedition from the Geological Department of Princeton University, has been placed in my hands for study through the courtesy of Prof. W. B. Scott and Dr. A. E. Ortmann. Although the collection contains only half a dozen species, it is of more than usual interest on account of the excellent preservation of the fossils and the fact that they probably come from either a new horizon for fresh-water mollusks, or at least a new

[1] *Motu Grammar* (Sydney, 1896), p. 9.

basin, as is indicated by their apparent distinctness from all described species.

According to the labels the fossils all come from one locality on Wettacombe's ranche near Harlowton, on the Musselshell River, Montana, where they were collected by Dr. M. S. Farr and Mr. A. Silberling. The interesting Mesozoic and Tertiary section of this region lying in Sweetgrass county, east of the Crazy Mountains and south of the Big Snowy Mountains, has been somewhat fully described by Mr. Earl Douglass,[1] who states that the Fort Union, the Laramie and the familiar Meek and Hayden section of the marine Upper Cretaceous are well represented. Beneath the Fort Benton formation is a thick series—"many hundreds of feet"—of sandstones and shales, of which the upper part is supposed to represent the Dakota and the lower part—"largely red in color"—yielded bones of large Dinosaurs, fossil wood and these invertebrates. Douglass refers this part of the section with doubt to the Jurassic, though he states that the vertebrate remains have not been studied. I have not been informed as to whether the mollusks and vertebrates occur in exactly the same bed. Apparently the exposure does not extend to beds as low as the marine Jurassic, which is known to occur in this general region, and which belongs to the upper part of the Jurassic system.

It is evident from the above statement that the fresh-water horizon in question lies somewhere between the marine Upper Jurassic and the Fort Benton, which may be correlated with the Turonian of Europe. This interval, covering the lower part of the Upper Cretaceous, all of the Lower Cretaceous and possibly the latest Jurassic, is not represented by marine strata in the northern interior region. Instead there is a number of non-marine formations in various parts of the region, whose relationships to each other are obscure, their principal point of resemblance in most cases being an apparently similar stratigraphic position.

Since in other parts of the continent this interval includes the equivalent of the Comanche series, consisting of several thousand feet of marine sediments and containing a number of distinct faunas, it is evident that there is room for many distinct horizons of fresh-water beds, and it would not be surprising if those of different ages were in some cases developed in different parts of the

[1] *Science*, n. s., Vol. XV, pp. 31 and 272, January 3 and February 14, 1902; *Proc. Amer. Philos. Soc.*, Vol. XLI, pp. 207–224, 1902.

large area, so that their exact stratigraphic relations with each other are not observable. It will therefore be necessary to consider the different formations and horizons that have been recognized, in order to make an approximate determination of the age of the fossils now under consideration.

In southern Wyoming the marine Jurassic is immediately overlain by the Como beds (formerly called Atlantosaurus beds), containing a large reptilian fauna and a considerable number of Unios and other fresh-water shells. Similar beds that are correlated with them by means of the fossils occur in the Black Hills, along the Front Range in Colorado and elsewhere in the Rocky Mountain region. These beds have usually been referred to the Jurassic, though recently several paleontologists have referred them to the Lower Cretaceous. The mollusca are of modern types, mostly belonging to genera that are still represented by living species, but the specific forms are quite distinct from all those found fossil in later beds. Of all the species that have been assigned to the Como horizon only one (*Viviparus gilli* M. and H.) is comparable with a form in this Montana collection, and that one is from a locality near the head of Wind River, Wyoming, where it was associated with *Liaplacodes veternus* M. and H. and *Neritina nebrascensis* M. and H. As none of these three species has been found elsewhere,[1] the age of the bed from which they came is doubtful, and may well be later than the Como.

Two non-marine formations of this general region, the Cascade and the Lakota,[2] have been referred to the Lower Cretaceous. The Cascade formation, which occur in the neighborhood of Great Falls, Montana, is coal-bearing and has been correlated by means of the fossil plants with the Kootanie of the neighboring Rocky Mountain region in Canada, and with the lower Potomac of the Atlantic border. Its geographic position and the apparently similar stratigraphic relations favor the supposition that the fresh-water beds near Harlowton may belong to the Cascade (Kootanie) formation, but unfortunately the former have yielded no plants except fossil wood, and the fauna of the latter is practically unknown. Obscure imprints of Unio have been reported from the Cascade, but nothing sufficiently definite for description. A few undescribed

[1] Logan inadvertently describes *Planorbis veternus* as *Liaplacodes* from the Freeze-out Hills of Wyoming, in *Kansas Univ. Quart.*, Vol. IX, p. 132, 1900.

[2] Named by Weed in the Fort Benton Folio, *Geol. Atlas of the United States.*

fresh-water gastropods have been collected from beds immediately overlying the Cascade and referred by Mr. Weed to the Dakota, and these are of types different from any in the Harlowton collection. That is, they are not specifically comparable.

The Lakota formation is found in the Black Hills region, where it is said to overlie beds correlated with the Como beds, and to underlie the Dakota. It is characterized by a flora, by means of which its Lower Cretaceous age has been determined, and it has been tentatively correlated with a part of the Potomac and by inference also with the Kootanie and a part of the Glen Rose beds. It has yielded no animal remains, and therefore needs no further mention in this connection.

Beds that have been referred to or correlated with the Dakota have a wider distribution than any of the other formations above mentioned. The original area is in northeastern Nebraska on the Missouri River, from which the formation has been satisfactorily traced and identified through Nebraska and Kansas, where it covers considerable areas on the Great Plains. It consists of several hundred feet of coarse sandstone with some shales, passing up into the Fort Benton shales with evident continuity of sedimentation. Paleontologically it is chiefly characterized by its large flora. It has yielded a very few marine and brackish water mollusca, but although it was evidently deposited at or near sea level it seems to have been largely non-marine in character, since a number of localities in it have yielded fresh-water mollusca. The abundant land flora also indicates non-marine conditions. The fauna[1] includes species of Unio, Margaritana, Corbula, Goniobasis, Viviparus and Pyrgulifera, none of which is specifically closely related to the species described in this paper. The Dakota has also been well identified in the Black Hills region and along the eastern base of the Rocky Mountains in Colorado. It has been mapped in many areas farther west in Wyoming, Montana, Utah, and elsewhere, but the correlation is certainly erroneous in some of these areas and must be considered doubtful in many of them, because the identifications have been based entirely on very general comparisons of the lithology and stratigraphy. It has been the general custom to refer to the Dakota any formation consisting in part of conglomerates and sandstones and underlying the

[1] See C. A. White, "Notes on the Invertebrate Fauna of the Dakota Formation," *Proc. U. S. Nat. Mus.*, Vol. XVII, pp. 131–138, 1894.

marine Cretaceous. That the formations thus assigned in many cases include the equivalent of the true Dakota is very probable, but that they may also include other horizons, laid down in the long interval between the Jurassic and the Upper Cretaceous, is equally probable. In the Yellowstone Park and adjacent areas the supposed Dakota includes bands of impure limestone filled with fresh-water shells not found elsewhere. Among them is a Unio represented by rare casts and fragments, but the most of them are simple gastropods, the most common of which I have described as *Goniobasis ? pealei*, *G.? increbescens* and *Amnicola ? cretacea*.[1] The beds referred to the Dakota near Great Falls, Montana, already mentioned, contain another assemblage of three or four species of fresh-water mollusca not known elsewhere. None of these supposed Dakota beds in the region just mentioned yielded the characteristic Dakota flora.

The Bear River formation of western Wyoming is the last one to be considered in this connection. It consists of a great thickness (as much as 4000 feet in some sections) of conglomerates, sandstones and shales, having a large and peculiar fresh-water fauna. Its principal known area extends from the neighborhood of Evanston, on the Union Pacific Railroad, northward near the western boundary of Wyoming for more than a hundred miles. Originally it was assigned to the Tertiary, afterward to the Laramie, or uppermost Cretaceous. It is now known to lie between the Fort Benton and the marine Jurassic,[2] but just how much or what part of this interval it represents is not positively known. Some of the conglomerates associated with the fossiliferous beds have probably been mapped as Dakota by the early surveys. The occurrence of a few undetermined dicotyledonous plants of modern type in the formation favor its assignment to the Upper Cretaceous. The fauna is not closely related to any other known on this continent, as all the species are restricted to it, and at least two of the gastropod types are so peculiar that they have been described as new genera. The entire fauna has been reviewed and figured by Dr. C. A. White,[3] who has made detailed comparisons with other non-marine faunas. One of the most striking of the common fossils of the fauna is *Pyrgulifera*

[1] *Monog. U. S. Geol. Surv.*, Vol. XXXII, Pt. 2, pp. 632, 633, 1899.

[2] See Stanton: "The Stratigraphic Position of the Bear River Formation," *Am. Jour. Sci.*, 3d ser., Vol. XLIII, pp. 98–115, 1892.

[3] *Bull. U. S. Geol. Surv.*, No. 128.

humerosa Meek, and the genus to which it belongs was not known to occur in America outside of the Bear River formation until a few years ago, when a species apparently referable to it was described from the Dakota of Nebraska, and another undescribed species is now known from the same formation in Kansas. This fact is an additional indication that the Bear River and Dakota are of nearly the same age.

It is worthy of note that the Bear River fauna includes an Ostrea, which proves that the formation was deposited at or near sea level, and that the waters occasionally became brackish, at least locally. The geographic distribution of the older marine formations makes this occurrence of Ostrea still another reason for considering the Bear River an Upper Cretaceous formation.

The six species of invertebrates from near Harlowton will be described on succeeding pages under the following names:

Unio farri.
Unio douglassi.
Campeloma harlowtonensis.
Viviparus montanaensis.
Goniobasis ? ortmanni.
Goniobasis ? silberlingi.

These are all new specific names, and it will be necessary to depend on the known range of the species that appear to be most closely related in attempting to determine the age of the beds from which they came.

Two of the species, *Unio douglassi* and *Campeloma harlowtonensis*, have their nearest relatives in the Bear River formation. *Unio douglassi* is of the same general type as *U. vetustus* Meek and has very similar beak sculpture, though it differs considerably in outline and proportions. *Campeloma harlowtonensis* resembles *C. macrospira* Meek so closely that it is difficult to separate them.

Viviparus montanaensis, as has already been stated, is closely related to *V. gilli* M and H., which has been doubtfully referred to the Jurassic.

The other species of Unio is of modern type, but apparently not very closely related to any known fossil species, while the two forms that are here referred to Goniobasis have a very modern aspect, suggesting Upper Cretaceous rather than older types.

Although the evidence is not fully convincing, the indications are that this fresh-water horizon near Harlowton is not far from the hori-

zon of the Bear River formation—possibly contemporaneous with a part of it—and that it is certainly not older than Lower Cretaceous, and more probably should be assigned to about the base of the Upper Cretaceous.

The facts thus briefly related should call renewed attention to the important and apparently complex history recorded in the non-marine formations of the late Jurassic and early Cretaceous of the Northwest—a history that is as yet far from being fully understood, although it is evident that the deposits contain the record of a great many facts that await the detailed investigation of the region for their interpretation.

Description of Species.

Unio farri n. sp. Pl. IV, Figs. 1, 2.

Shell small, short and relatively convex; beaks somewhat prominent and inflated, situated about one-third the length of the shell from the anterior end, not sculptured nor eroded; dorsal margin nearly straight; ventral margin moderately convex; anterior end regularly rounded; posterior end obliquely subtruncate; umbonal ridges rather prominent, rounded, extending to the postero-ventral angle; surface marked only by moderately prominent, irregularly arranged lines of growth.

The type specimen measures 37 mm. in length, 22 mm. in height, and 16 mm. in convexity of both valves. The largest specimen of the eight in the collection is 45 mm. in length. Three of the specimens are relatively somewhat more compressed and longer and have the posterior end more obliquely truncated. These differences are believed to be sexual rather than specific and are not greater differences than are seen in some living species of Unio. One of these compressed specimens (represented by Fig. 2) measures 40 mm. in length, 22 mm. in height and 13 mm. in convexity.

This species is suggestive of the *parvus* group of living Unios, but is not sufficiently closely related to any fossil species from our Western formation to require detailed comparison.

Locality.—Wettacombe's ranche near Musselshell River, in the vicinity of Harlowton, Montana.

Horizon.—Upper part of Lower Cretaceous or base of Upper Cretaceous.

UNIO DOUGLASSI n. sp. Pl. IV, Figs. 3, 4.

Shell below medium size, elongate, rather slender and moderately convex; beaks small, inconspicuous, situated about one-fourth the length of the shell from the anterior end; dorsal margin nearly straight; ventral margin very gently convex; anterior end regularly rounded; posterior end broadly rounded below and very obliquely subtruncate above, so that its termination is acutely subangular; umbonal region strongly sculptured over an area about 17 mm. long and 7 mm. high, with prominent concentric ribs, crossed on the posterior portion by two sharply elevated, linear, radiating ribs, one of which is on the umbonal ridge and the other midway between it and the postero-dorsal margin. The concentric ribs change their direction abruptly and have their continuity more or less broken in crossing the radiating ribs, especially the upper one. The rest of the shell shows only moderately distinct growth-lines, except on the postero-dorsal area above the umbonal ridge, which shows numerous faint, slightly curved, irregular radiating lines.

An average specimen measures 56 mm. in length, 24 mm. in height and 15 mm. in convexity (both valves), the greatest height and convexity being about midlength of the shell. A few specimens appear to be relatively somewhat more compressed and higher, but this is due, in part at least, to accidental distortion. The species is represented by about forty specimens.

This species closely resembles *Unio vetustus* Meek from the Bear River formation in all the details of sculpture, but it differs from that species in its smaller size and much more slender form.

Locality.—Wettacombe's ranche near Musselshell River, in the vicinity of Harlowton, Montana.

Horizon.—Upper part of Lower Cretaceous or base of Upper Cretaceous.

VIVIPARUS MONTANAENSIS n. sp. Pl. IV, Fig. 5.

Shell small, rather stout, subovate, consisting of about five rapidly increasing whorls; volutions rounded below and obtusely subangular and flattened above, so that they are more or less distinctly shouldered; last volution slightly expanded at the aperture, which is broadly ovate; outer lip simple, nearly straight in profile outline; inner lip moderately thick, closely appressed to the shell above, very slightly elevated and reflexed below; surface bearing only very fine lines of growth.

The figured type, which is of about the average adult size, gives the following measurements: Height, 12 mm.; greatest breadth, 10 mm.; height of aperture, 7 mm.; breadth of same, 5 mm. The collection contains about two hundred specimens, which show little variation except in size. Part of the specimens, supposed to be immature, are considerably smaller than the type.

This species is very similar in general appearance to *Viviparus gilli* Meek and Hayden, which was described[1] from beds provisionally referred to the Jurassic at the head of Wind River, Wyoming, but it differs from the Wyoming form in being slightly smaller, and in having more distinctly shouldered whorls, a more oblique aperture, and with the inner lip more closely appressed to the shell and not so prominent.

Locality.—Wettacombe's ranch near Musselshell River, in the vicinity of Harlowton, Montana.

Horizon.—Upper part of Lower Cretaceous or base of Upper Cretaceous.

CAMPELOMA HARLOWTONENSIS n. sp. Pl. IV, Figs, 11, 12.

Shell large, elongate subovate, consisting of about six elevated convex whorls, separated by a linear impressed suture; aperture large, ovate; inner lip moderately thick, forming a rather heavy callus on the shell above and slightly reflexed below, so as to partly cover the small umbilical depression or chink; surface marked only by fine, slightly sigmoid lines of growth.

The type, which is the largest specimen in the collection, measures 63 mm. in height (with apex restored) and 38 mm. in greatest breadth; height of aperture 34 mm. and breadth of same 23 mm. The other figured specimen is 57 mm. in height (with apex restored), 38 mm. in breadth, and the corresponding dimensions of the aperture are 29 mm. and 22 mm. respectively.

This second specimen shows other differences in the aperture besides its relatively greater breadth, the inner lip being thicker and not so closely applied to the preceding whorl above and having a larger umbilical depression uncovered below. These variations are probably all accidental, as there is distinct evidence of injury to the shell during the life of the animal, shown by a repaired break nearly parallel to the outer lip and a few millimeters from it.

[1] *Palaeont. of the Upper Missouri*, p. 115, Pl. V, Fig. 3, *a*, *b*, Washington, 1865. Figured also by White in Bull. No. 29 and in *Third Ann. Rept. U. S. Geol. Surv.*

There are seventeen other less perfect specimens in the collection, all of which agree fairly well with the type so far as their characteristics are preserved.

This species is very closely related to *Campeloma macrospira* Meek,[1] and may prove to be not more than a variety of that species from the Bear River formation in western Wyoming. Comparison of *C. harlowtonensis* with the type and with a large suite of specimens from the same horizon in Wyoming show that Meek's species averages considerably smaller, and that it is somewhat more slender, with the sutures slightly more oblique and the last whorl relatively larger. The last-named peculiarities cause a greater difference in the aspect and proportions of the shells than would be indicated by measurements. There are also differences in the form of the inner lip. There are, however, associated with the typical form of *C. macrospira* a few specimens that approach more closely to the form here described, and this fact suggests the question whether there are really two species, or only varieties of one variable species.

Locality.—Wettacombe's ranch, near Musselshell River, in the vicinity of Harlowton, Montana.

Horizon.—Upper part of Lower Cretaceous or base of Upper Cretaceous.

GONIOBASIS? ORTMANNI n. sp. Pl. IV, Figs. 7–10.

Shell small, moderately slender, consisting of about six convex whorls; aperture elongate ovate, slightly produced below; inner lip somewhat thickened, closely appressed to last whorl above, slightly reflexed below so as to partly cover the small umbilical chink; surface bearing inconspicuous growth-lines, usually crossed by a variable number of much more prominent sharply elevated spiral lines, which in some cases are strong enough to be called small carinæ. Specimens which may be considered to have the average or typical sculpture show four spiral lines on the whorls of the spire, with about six additional on the base of the last whorl

[1] Named by Meek in a list without description in *Rept. U. S. Geol. Surv. Terr.*, for 1872, p. 478. Described in 1877, *U. S. Geol. Expl. 40th Parallel*, Vol. IV, pt. 1, p. 180, with figures of a small shell doubtfully referred to the species. Figures of Meek's original type are published by White, *Twelfth Ann. Rept. U. S. Geol. Surv. Terr.*, Pl. 30, Fig. 2*a*; *Third Ann. Rept. U. S. Geol. Surv.*, Pl. 8, Figs. 6, 7, and *Bull. U. S. Geol. Surv.*, No. 128, Pl. 10, Figs. 2, 3.

and sometimes a few finer intermediate lines. A few individuals show five lines on the spire, while others have only three or two. Smooth forms like that represented by Fig. 10 usually show incipient spiral lines on the last whorl.

Height of an average specimen, about 17 mm.; greatest breadth, 8 mm.; height of aperture, 7 mm.; breadth of aperture, 5 mm.

The most striking feature of the species is the variability of its sculpture, though in this respect is comparable with such living species as *G. virginica* Gmelin. Of about 200 specimens in the collection nearly half either lack spiral sculpture or have it very faintly developed.

The generic reference of Goniobasis is not entirely satisfactory, as the aperture differs in some respects from typical living species of the genus. It slightly suggests *Lioplacodes veternus* Meek from the supposed Jurassic at the head of Wind River, but it is specifically very distinct and I think not referable to the same genus. In sculpture it resembles *G. tenuicarinata* M. and H. from the Laramie more closely than any other fossil form.

Locality.—Wettacombe's ranch near Musselshell River, in the vicinity of Harlowton, Montana.

Horizon.—Upper part of Lower Cretaceous or base of Upper Cretaceous.

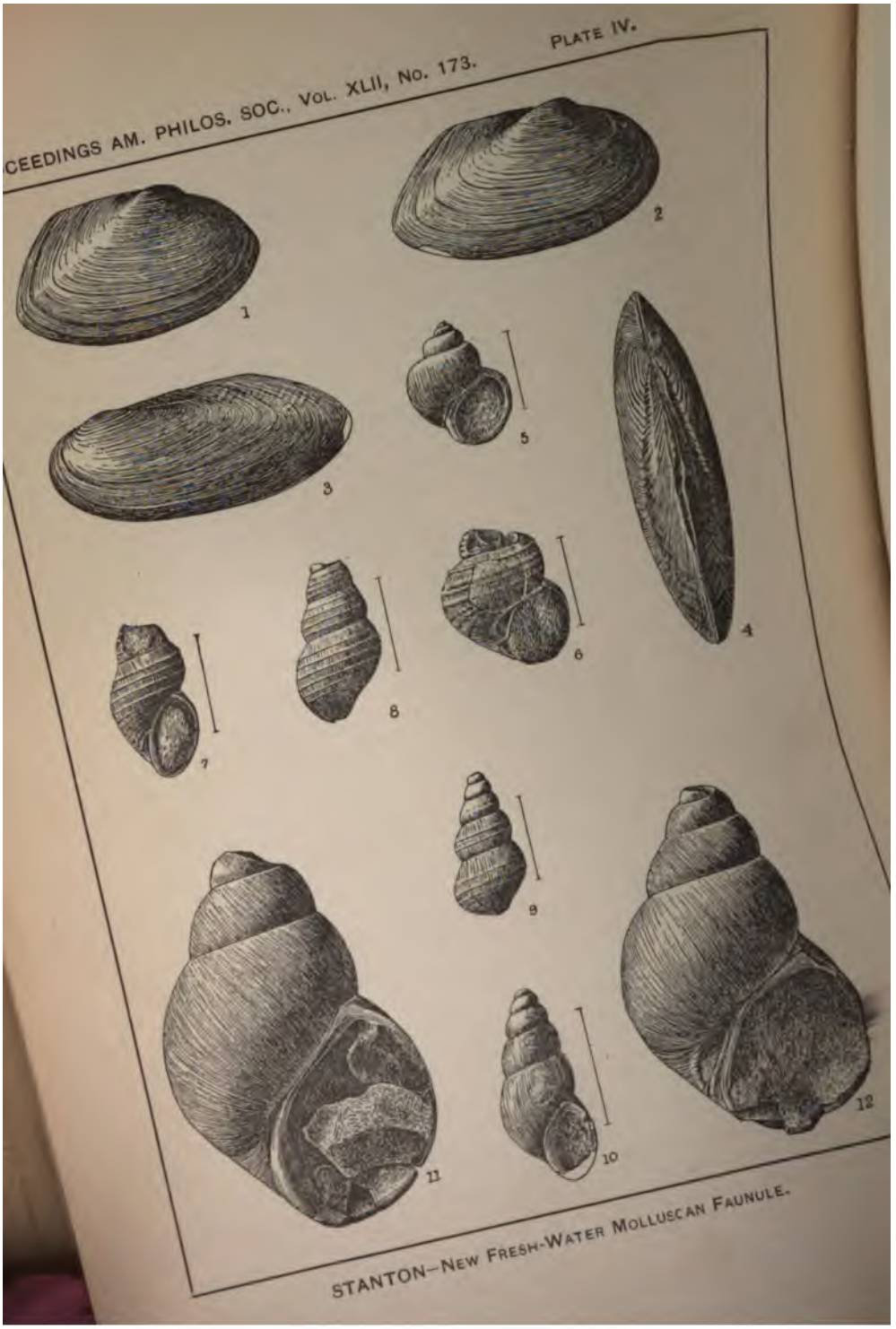
GONIOBASIS? SILBERLINGI n. sp. Pl. IV, Fig. 6.

A single fragmentary specimen associated with the preceding seems to be worthy of description, although the generic reference is very doubtful. It is the basal portion of a shell consisting of nearly two whorls and may be described as follows:

Shell of moderate size, rather stout; whorls very convex; aperture broadly ovate; inner lip thin, slightly reflexed below over a distinct umbilical pit; surface of the spire with four strong spiral ridges or carinæ, which are unequally spaced, the space between the uppermost one and the suture and also between it and its neighbor being broader than the other smooth bands.

The fragment measures 13 mm. in height and 13 mm. in greatest breadth; height of aperture, partly estimated, 9 mm.; breadth of same, 6 mm.

The base of the aperture is broken, and it is possible that the large size of the umbilical pit is due to abnormal individual development. If this is a normal example of the species, it can hardly



STANTON—NEW FRESH-WATER MOLLUSCAN FAUNULE.

be placed in the same genus with *G.? ortmanni*. In sculpture it suggests the carinated forms that White has very doubtfully referred to *Lioplax endlichi* from the Bear River formation.

Locality.—Wettacombe's ranch, near Musselshell River, in the vicinity of Harlowton, Montana.

Horizon.—Upper part of Lower Cretaceous or base of Upper Cretaceous.

EXPLANATION OF PLATE.

Unio farri Stanton.

Fig. 1. Right valve of type.

Fig. 2. Right valve of compressed form, probably male.

Unio douglassi Stanton.

Fig. 3. Left valve of a small specimen.

Fig. 4. Dorsal view of an average-sized specimen.

Viviparus montanaensis Stanton.

Fig. 5. Aperture view of the type, enlarged.

Goniobasis? silberlingi Stanton.

Fig. 6. Aperture view of the type, enlarged.

Goniobasis? ortmanni Stanton.

Fig. 7. Aperture view of fragmentary specimen with strong sculpture, enlarged. Outer lip restored from another specimen.

Fig. 8. Dorsal view of a similar specimen, enlarged.

Fig. 9. A specimen with only two spiral lines on the spire, enlarged.

Fig. 10. Aperture view of a specimen without spiral sculpture except on back of last whorl, enlarged.

Campeloma harlowtonensis Stanton.

Fig. 11. Aperture view of the type.

Fig. 12. Similar view of a broader, more umbilicated specimen.

REACTION AS AN EFFICIENT AGENT IN PROCURING DEEPER NAVIGABLE CHANNELS IN THE IMPROVEMENT OF RIVERS AND HARBORS.

BY LEWIS M. HAUPT, A.M., C.E.

(*Read April 2, 1903.*)

Consumption, production and distribution are the three main elements of trade. Without great facilities for distribution it is not possible to maintain a nice adjustment between supply and demand. One section of the earth may be starving, while another may be burning its excess of food for lack of cheap transportation.

The question, therefore, has its humane as well as its financial and scientific aspects. It is the aim of the engineer and the capitalist to reduce the cost of transportation to a minimum for the general welfare of mankind.

The great improvements which have been effected in the railways of the world have resulted in a rapid reduction in the average rates of freight, which are still falling. Roadmakers have caught the infection and are mending their ways as rapidly as the means become available. Sailing vessels are transformed into the schooner type of greater dimensions and are designed to be handled by smaller crews, so that it may be said they represent the cheapest class of carriers. The steamer also is being greatly enlarged in its capacity with the same end in view, but it has not and cannot reach the limit of its economic possibilities because of the absence of adequate channels at its terminals.

These great evolutions in transportation have been made possible in the United States by the concentration of mind, money and materials, working in harmony and resulting in a system of overland movements which is without a rival. It is the outgrowth of private capital, employed to develop limited areas, but gradually consolidated into trunk lines, and which finally, assisted by the National Government, united the two oceans. The merging of these great interests still continues and the end is not yet. These bands of steel have enabled our excess of production to reach the seaboard and be distributed to foreign markets, and it may not be out of order to glance very briefly at the magnitude of this movement.

Thanks to a beneficent Providence and the industry and intelligence of our people, our exports exceed our imports by an amount greater than that of all other nations. Their increase within a generation is startling. While the population has doubled in the past thirty years, the per capita of money has increased from $17.50 to $28.66.[1] The number of artisans has increased 2.7 times, while his average earnings have risen from $387 to $500 per capita per annum. The capital employed has expanded fivefold and the value of the output more than threefold. In consequence the per capita of our exports has increased in this same period from $7.29 to $18.81, of which the largest part is food-stuffs.

The increase in agricultural exports was over 300 per cent., and that of manufactures 750 per cent., so that this country heads the

[1] O. P. Austin, Chief of the Bureau of Statistics, in *The World's Work*.

list of exporting nations, having reached nearly one and a half billion dollars in 1901. The United States produces more wheat than any other country of the world; more corn than all other countries combined; more beef and pork than any other; three-fourths of the world's supply of cotton; of coal our exports exceed those of any other nation, and at far less cost. We are the mainstay of the world for petroleum for light, heat and other purposes, and we lead in the quantity and value of manufactured articles.[1]

For the internal distribution of the nearly 900,000,000 tons of traffic, resulting from our splendid resources and energies, we have over 200,000 miles of railways, or more than two-fifths of the world's mileage, to say nothing of the superior facilities for the distribution of thought by mail, telephone and telegraph. Thus it is seen that, although the population of the country has doubled within thirty years, the productivity of the nation has far exceeded this ratio, and that it is the main reliance of Europe for many of its necessities. Our importance as a base of supplies for the Orient is also rapidly increasing, and it is reasonable to suppose that the next generation will realize even greater developments than this.

The present year heralds the preparation which the great masters of transportation are making for "round-the-world" lines by the consolidation of the ocean carriers into the International Mercantile Marine Company, so that our exports may be delivered in vessels under domestic control at less cost, and our heavy freight bills to foreign flags be reduced. This is as it should be, and every possible encouragement should be given to all legitimate efforts to increase the circulation of material products and to reduce the cost, thus extending the market-range. But the mere multiplication in the number of vessels does not lower the cost unless it develops a keen competition between rivals. This competition is to some extent neutralized by combination, but under good management this effect may be more than offset by the reduction in fixed charges and by the use of vessels of greater tonnage, which can be operated at less cost per ton of cargo transported.

This brings us directly to the crux of the argument, for the vessels, having already outgrown their channels are obliged to await favorable conditions, clear with partial cargoes or lighter; in every case adding to the cost at the expense of the consumer, or restricting deliveries. It has been predicted that ere long vessels of 1000

[1] O. P. Austin, Statistician, Treasury Department, Washington.

feet length and forty or more feet draft would be upon us, but this economic ideal cannot be realized until some better method is developed for the creation and maintenance of much deeper channels. To meet this demand for deeper water more powerful dredges are building, in the hope of combatting successfully with the ceaseless activities of the bar-building elements, by sporadic mechanical devices, costing large sums to operate and offering serious obstacles to navigation by their presence in narrow channels. With the exception of Port Royal, with twenty-one feet; Gedney's Channel, with twenty-three feet; the Golden Gate, with thirty-two feet, and the Columbia Bar, with nineteen feet, the natural depth of scour over our alluvial bars seldom exceeds fifteen feet, and is more frequently limited to from three to twelve; while a modern vessel, fully laden, may draw thirty-two, and should have a channel depth of from thirty-five to forty feet for safe passage over a rough bar at low water. Hence the urgent demands made upon the national treasury for larger appropriations, that at least the most important of our railroad and commercial terminals may utilize these economies in transportation.

For the forty-four years prior to 1866, when our commerce was carried in much lighter-draft vessels, the total expenditure for waterway improvements was but $14,990,170; but between 1867 and 1901 they expanded to $332,487,627—making a grand total to that date of $347,477,897, to which should be added the appropriations of the last Congress of about $60,000,000 more, thus swelling the aggregate to over $400,000,000. In reporting the last bill the Chairman of the River and Harbor Committee stated that "the total amount which would be required for the completion of projects for river and harbor works . . . now considerably exceeds $300,000,000." If but ten per cent. of this sum can be secured annually it is evident that our commerce must "drag its slow length along" for many years, while the increase in the demand for greater facilities cannot be met unless greater efficiency may be secured in the methods in vogue.

During the score of years succeeding 1867 the average expenditures were $4,480,000, but soon thereafter, when deeper channels were demanded and the use of the submerged and twin jetties supplemented by dredging became the main reliance, the annual average reached nearly $13,000,000, with a rapidly increasing ratio. In the past quarter century the estimates and expenditures at only

eight of our most important seaports, where jetties have been built or proposed for channels of modern depth, foot up to $50,515,784, but the difficulty of securing the depth has necessitated in such cases a resort to dredging to create and maintain the channel. These new conditions have resulted in the construction of powerful sea-going hydraulic dredges with great capacity, and have in a measure revolutionized the practice of deepening by scour, as it is considered by some more economical to use the dredge without regulating works. In consequence it is found that the amounts expended and estimated to complete the approved projects at only four of our principal ports by dredging alone will aggregate $41,396,129, exclusive of the large additional sums required for maintenance.

In view, therefore, of the important interests involved, the unreliability of dredged channels, the inadequacy of twin jetties and the great cost, it would seem pertinent to inquire whether the profession of engineering has reached its ultimatum in this department of science. Is it not possible to utilize to greater extent the boundless resources of nature for the purpose of creating deeper channels at our ports?

The magnitude of these forces will be better understood when it is shown that the sun as a prime mover evaporates approximately 15,000 tons from each square mile of the ocean's surface every twenty-four hours, so that his daily work upon the 150,000,000 miles of water surface represents a load of two and a quarter trillion tons, a large portion of which is carried by the wind-driven clouds to the land where it is recondensed. Assuming the precipitation to be proportional to the ratio of land to water, there would be 562 billion tons falling on the land surface, and taking the run-off at but 40 per cent., there results 225 billion tons of stored energy flowing down to the sea every day of the year, or, reducing this weight to its volumetric equivalent, we have nearly fifty cubic miles or 264,000 square miles of water one foot deep, an area greater than the State of Texas.

This is the fluid solvent which, in the laboratory of nature, is daily applied to earth-sculpture, while the portion at work in the chemical and metallurgical laboratories of the interior is much greater than this. The former is all that is available for the avenues of domestic commerce, while the latter is the part which contributes to its tonnage by developing its products.

For the foreign commerce there is the illimitable ocean with its dynamics—the tides, winds and currents—which are not yet fully understood nor utilized.

The poet Milton has aptly said :

> "Accuse not Nature, she hath done her part;
> Do thou but thine."

This prompts the question, How? It is to answer this query that attention will be briefly directed.

It is well known that engineering, like many other sciences, is largely empirical, and that more is learned from failures than from successes, for failures are the buoys which mark the channel to success. It becomes important, therefore, to review the experiences of the past, in which this country is particularly rich, that their lessons may guide us in preparing to satisfy the demands of the future. With this end in mind, a brief review will be made of a few types of harbor improvements, showing their physical features and results, and the methods which have produced them.

Existing Methods.

The devices in use to-day for the alleviation of the evils of ocean bars are twin jetties, dredging and dynamite, either singly or combined. The theory of the two-jetty system has been so long and ably discussed that little need be added further than to state that it is based upon the idea of preventing a dispersion of the currents by the building up of parallel or convergent training walls to concentrate the discharge upon a single path across the bar.

The objections to this system are that, being built out from shore, the confined waters are projected upon the inner slope of the bar, which is pressed seaward as they advance. Moreover, being at a fixed distance apart, they cannot be adapted to great ranges of stage, for if adjusted to a normal low-water discharge they will be too close to pass the floods without retardation, or the reverse. In any case there must result a sedimentation above, within or beyond the works, as will be shown later, and dredging must be applied for relief, and, furthermore, they reduce the tidal influx. Until within a few years twin jetties aided by dredging have been the panacea for all classes of harbor bars, regardless of the relations between deposits, discharge and the many other conditions affecting their

formation and maintenance. It is important that a careful diagnosis be made of each case to ascertain its preponderating element.

Physical Considerations.

Thus it is seen that the physical agencies become of fundamental importance, and that a clear distinction must be made between bars formed from littoral drift and those formed from the detritus carried down by streams. For tidal inlets it is also important to ascertain the prevailing direction of the littoral movements which have frequently but erroneously been supposed to follow the prevailing winds, whereas it is more frequently found to be the resultant of the configuration of the adjacent coast line and of the angular wave movements, especially during the flood tide, when the waves are most heavily charged with silt.

Knowing the direction of this general resultant, the engineer can then determine on which side of the channel his protecting work should be placed, although there still seems to be a radical difference of opinion as to whether it should be on the near or far side, for only recently it was recommended that if a single jetty were built at a certain inlet on the Southern coast, it should "be located on the south of the channel, since the drifting sands come from the north. At this place, however, while the drift is comparatively slow, it is an enormous sand bank which moves, and which always moves very positively in one direction, and it is difficult to see how such a constant force from the north could avoid crowding the channel close to the jetty." The jetty plan was therefore rejected. Frequent experience in the construction of two jetties, where the farther one has been built in advance of the nearer one, has served to show the fallacy of this location and order of procedure.

The requirements to be met at tidal inlets are, free admission of the flood tide as the only source of ebb energy, protection of the bar channel from the prevailing direction of the littoral drift, conservation of ebb tide as it passes seaward over a narrower path on the bar, development of its potential energy in useful work locally on the bar crest and an automatic adjustment to any stages of wind or tide. All of these may be better fulfilled generally by one jetty than by two, and manifestly at about half the cost. These results are rendered possible by placing in the way of the ebb current a curved resisting medium in such position as to maintain a continuous reaction along its concave face. In fine, this structure be-

comes the tool for the conversion of the effluent energy into useful work, with lateral transportation of the eroded material.

Reaction versus Velocity.

The opinion is prevalent that the deep pockets frequently observed at the ends of spurs or obstacles or at contractions in rivers are due to velocity, and it has been stated that because a mean ebb velocity of two feet per second maintains a depth of over 100 feet at the Narrows of New York Harbor, therefore a similar contraction on the bar near Sandy Hook would produce some such depths. This was made the basis in 1886 for a proposition to build a jetty nearly five miles long, closing three of the channels across the New York bar. The great depth at the Narrows is not a velocity but a reaction depth, due to the resistance which the converging shores oppose to the passage of the flood, not the ebb tide, which increases the head before reaching the pass, depressing the resultant to the bottom, from which it reacts and scours out a depth to compensate for the lateral contraction. At other points in the harbor velocities of more than two feet per second do not scour to depths exceeding three feet, so that the results must be ascribed to some other cause than mere velocity.

An extended investigation of these abnormal depths leads to the conclusion that they are caused by eddies operating in a vertical plane, these eddies being caused by obstacles placed in the path of a current in such manner as to retard the flow by the interference due to converging forces, thus creating a head with a downward resultant and scour until compensation is secured by enlarged aperture. Here the reaction produces a change of direction of the resultant, which is deflected upward with dispersion of energy, deposit of material and ultimately restored equilibrium. These facts are doubtless well known to many observers, but the particular point to which attention is directed in this connection is that the downward movement producing scour is supplemented necessarily by the upward resultant, accompanied by deposit in the same vertical plane, so that whichever way the eddy operates, whether with flood or ebb, the bar is a sequence of the pocket, unless other forces come to the rescue. Thus it appears that an eddy both scours and deposits. These effects are reciprocal results of the same eddy, and not of two separate ones. But eddies also operate in horizontal planes, and with like results. When the obstacle is limited in extent the effect

is local, but when the resistance is maintained the reaction continues to be developed and the energy to be expended until the resistance ceases.

The great advantages resulting from a continous reaction produced by a concave directrix appears to have been largely ignored in the work of river and harbor improvement, and yet the location of the best channels under the concave banks of rivers attests its value to commerce. It is true that numerous curved dikes and revetments have been placed in the concave bends of rivers, but the object has been to protect them *from* erosion and not to encourage it. Their value as tools to cut away an ocean bar does not appear to be fully appreciated, since where single curved jetties have been built the convex face has generally been turned to the current, to encourage, as has been said, the tendency which water has to follow a convex curve. (?)

The concave directrix has also the great advantage of maintaining the head due to centrifugal force and thus changing the direction of the resultant downwardly, producing the lateral scour and resulting convex bank or counterscarp created by the stream acting as an hydraulic auger, and of automatically adjusting this counterpart to the variable requirements of its regimen.

These general principles will be more fully elucidated by illustrations selected from surveys and models, showing the holes bored by reaction and the shifting of channels by artificial works, which are instructive as to the intimate relation between cause and effect.

A study of the natural effects found to exist under certain conditions enables the engineer to predict with some assurance the results which may follow a utilization of the available forces at any site. One of the most instructive examples of the vertical eddy is to be seen in the Narrows at New York, to which reference has already been made. Here the bottom currents are with the flood tide for about eleven hours out of the twelve, and this resultant flood extends as far up as the Battery. The ebb resultants are greatest at the surface and diminish rapidly with the depth, reaching their point of reversion at or near forty feet in the Narrows. On the bar the ebb currents show a feeble resultant at a depth of less than twenty-four feet in but one of the channels.

The remarkable "slue" which has maintained its position athwart the path of the currents since the earliest surveys has excited some attention as to its phenomenal position and depth of fifty-two feet.

It is referred to in the early Coast Survey Reports, and was made the subject of a special paper by the late honored member of this Society, Prof. Henry Mitchell, who prepared a manuscript report upon it, in connection with the physics of the lower bay, in 1858, but which was not published. It serves to confirm the claims of this paper that depths may be and frequently are the result of eddying action rather than velocity. The confluence of three currents produces a resultant having a northeasterly set which impinges upon the bar at the head of Gedney's Channel and is deflected thence by this resisting bank of sand northwardly, boring out the slue for a length of a mile and a width of a half mile. The latest survey shows a depth of fifty-three feet, with but eighteen feet on either flank. It was proposed at one time to change the direction of this resultant by cutting off one of its components and training the currents seaward to open Gedney's Channel by the utilization of this force, but it was not accepted. Again, the reaction at the head of Sandy Hook has produced a maximum depth of sixty-eight feet, diminishing within about a mile to thirty feet, while abreast of the point and a half mile distant the depth is but sixteen feet.

The construction of the old Breakwater at the mouth of the Delaware in 1828 furnishes some instructive lessons as to the changes effected by obstacles placed in a tideway. Here, at the ends of the ice-breaker and of the breakwater, are to be found the characteristic deep holes resulting from the head generated by the resisting structures. At the gap the pockets are on the outside of the opening, and the depths are the effects of the flood-tide. Both of these pockets are fifty feet deep, and the material scoured out from them has been carried into the harbor and deposited in the lee of the structures, making a shoal with only ten feet at one point. At the southeastern end of the breakwater the ebb reaction has scoured to a depth of fifty-four feet, while at the western end of the ice breaker the hole is due to the flood, and is limited to about forty feet. Moreover, the diagrams of velocity curves show that in the centre of the harbor, where the maximum velocity of the ebb is six feet per second, the bottom has not been prevented from shoaling to about fifteen feet, while a similar ebb velocity at the "gorge" is able to maintain depths of thirty feet. If these depths are due solely to velocities they should be equal, since like causes should produce like effects.

Numerous other instances of these abnormal depths due to reac-

tion, and not to velocity simply, might be cited, but a few must suffice.

In the Thoroughfare at Longport, N. J., the landing pier has caused a hole forty-eight feet deep, while 800 feet away there was a bar bare at low water, but covered by a tidal current almost as swift as that past the pier.

In the Charleston gorge the maximum depth was eighty-two feet, while on the bar seaward thereof the depth was zero, and the best crossing was six miles south of the gorge. At Fernandina (Cumberland Sound), Ga., the maximum depth at the head of Amelia Island, projecting into the channel, was sixty feet, and abreast of it bare at low water.

The Galveston gorge shows about fifty-eight feet, while the normal bar depths were twelve to thirteen. The St. John's river, Fla., swings to the sea through a radius of one mile, carrying a maximum depth of fifty and three-tenths feet, and as the axis straightens to a tangent the depth diminishes to twenty feet. It then strikes a jetty at an abrupt angle which develops its latent energy and scours to fifty and two-tenths feet, but as this is not maintained by the convex curve of the jetty, the channel deteriorates to about eleven feet.

The building of a spur in the Mississippi river at right angles to the bank had the effect of increasing the depth from twelve to nearly one hundred feet in consequence of the violent eddy which was created, and now that the spur is covered and the river has assumed a new regimen, the depth has shoaled to about thirty feet, which is maintained.

From these few instances it would seem to be a fair inference that depths may be developed quite as well by single lines of concave directing works as by two, if proper attention is given to the volume of affluent as well as to the relative amount and direction of the motion of the bar-building materials.

Practical Suggestions.

In view of the requirements as previously stated, it is evident that to protect the proposed channel from the littoral drift a submerged low-tide or half-tide jetty will not suffice to arrest this drift, but it should extend above the highest tide. It must also be placed between the channel and the source of the prevailing drift, just as a

snow- or a sand-fence must be placed to "windward" to protect a rail- or wagon-way.

It must be curved, concave to the effluent currents, to develop a continuous reaction, and should be constructed inward from the outward slope of the bar, to avoid the advance of the crest and to utilize the force of gravity in cutting shoreward and downward, instead of seaward and upward. These are some of the conditions which give promise of the greatest results attainable at a given location. Taking now a few typical illustrations of the several methods in vogue, it is seen that the New York entrance is to be deepened by dredging some 42,000,000 cubic yards from the bar, beginning at the easterly end of Ambrose Channel, at a cost of about $4,000,000, but with no definite time limit.

Although this sector of the bar shows a remarkable degree of permanency, it can hardly be expected that the formation of this deep cut, created by artificial means in the open sea, at a point where the natural depths are steadily maintained at from sixteen to eighteen feet, will long remain open. If it be assumed that normal conditions would be restored, say, in a period of ten years, it would represent an annual accretion of about 4,000,000 cubic yards to be removed by dredging, which at the present price would cost $360,000, or the interest at three per cent. on $12,000,000. For a much smaller sum it would be possible to train the currents through this new and shorter channel by permanent works which would maintain it and at the same time become a valuable aid to navigation.

The effects of the submerged jetty type is best seen at Charleston, where the littoral drift is southward. Here the outer ends of the jetties are raised above high water, but the shore flanks are far below the surface, to admit the tides freely. The result is that the beach sand travels across them and forms shoals within the harbor, while it also travels around the outer end and maintains a bar in the open sea more than half a mile beyond the works, through which the channel must be maintained by dredging.

At Galveston, where the submerged plan was modified to reach above high water, the building out of the south jetty first has caused the bar to advance some three miles, adding that length to each of the jetties, which are 7000 feet apart, and a new bar is forming across the mouth in the lee of the north jetty. The channel must also be maintained by dredging.

At the mouth of a sedimentary river, like the Mississippi, where

the silt comes from within, two jetties give much greater promise of success, and the great work of Captain James B. Eads in opening the South Pass by parallel jetties curving to the westward has proven to be a boon to the country. These jetties were built under adverse conditions, as payments were conditioned upon results to be secured, and as the first pair of jetties did not suffice to give the requisite depth, it became necessary to build spurs, then a second line of works, and finally a second series of spurs before the legal depths were obtained. This, however, resulted in an over-contraction of that outlet, and has caused the retarded currents to drop their sediment in the Pass above the jetties instead of beyond them, involving dredging.

Probably the most successful work of this kind at a river's mouth is that completed at the mouth of the Panuca river, Tampico, Mexico, where in two years' time two parallel straight jetties were constructed about a mile and a quarter long across a bar, having depths varying from five to twelve feet, and as soon as they were finished a severe flood flushed the channel so completely that the depths of twenty-seven feet have remained ever since, the littoral current here being sufficiently strong to remove the sediment carried out beyond the jetties. The engineer of this work was E. L. Costhell, C.E.

At Aransas Pass, a purely tidal inlet on the Texas coast, a single reaction breakwater, in an incomplete condition, has produced a progressive deepening by the control of feeble tides, unaided by dredging, and at a cost of less than one-third that of the estimated project, thus fully demonstrating the great practical utility of the single reaction jetty system at the only point where an opportunity has been afforded for a test on a large scale in this country, after about fifteen years of persistent effort The Consulting Engineers of this work were Messrs. H. C. Ripley, Geo. Y. Wisner, and the writer.

THE WARFARE AGAINST TUBERCULOSIS.

BY MAZYCK P. RAVENEL, M.D.

(*Read April 4, 1903.*)

It would seem almost superfluous to dwell on the terrible destruction of human life due to tuberculosis, or to dilate on the urgent necessity that exists for general and combined efforts to lessen its ravages; yet, in spite of all that has been said and written on the subject during the last ten years, and in spite of certain encouraging signs of awakening interest in the public mind, it is still true that there exists a lamentable and inexplicable apathy in regard to this scourge of the human race. Philanthropists are lavish in their gifts to colleges, hospitals, libraries, museums and such like institutions, yet in America at least there have been very few substantial donations toward the eradication of tuberculosis, though it would be hard to imagine a greater boon to stricken humanity than the accomplishment of this end. Legislators give freely to all kinds of charitable institutions, but the amount given to the army of the tuberculous is pitiably small. This attitude of legislators may to a great extent be taken as indicative of public sentiment. This sentiment becomes harder to understand when we consider that three facts have been absolutely demonstrated in regard to the disease: 1. It is communicable. 2. It is preventible. 3. In the early stages it is curable.

It is well to inquire into the causes of this lack of interest, and see if there is sound reason for them. One of the greatest drawbacks has been the persistent belief in the hereditary character of the disease, which is even now quite prevalent among the masses, and held by many physicians. While it has been shown that tuberculosis may be transmitted in this manner, it has been equally proven that it is of very rare occurrence, and practically negligible. Among the lower animals healthy offspring may be constantly obtained from tuberculous mothers by separation at birth and artificial feeding—a plan carried out on a large scale in Denmark by Prof. Bang. It is, however, true that tuberculosis runs in families, the reason being that the children of phthisical parents are constantly exposed to infection. In man only some twenty cases of true hereditary tuberculosis are on record (Osler), and in cattle there are less than 100 to be found in the literature. Another obstacle to progress is the inevi-

table tendency of the human mind to grow accustomed to danger. We have grown accustomed to the death-rate from tuberculosis, and do not realize what it means. A panic would be caused in any one of our large cities by 100 deaths from cholera, yellow fever or plague, yet in New York 10,000 deaths and in Philadelphia 2800 deaths are caused each year by tuberculosis without exciting even passing comment from the average person. The only real difference is that tuberculosis is with us always, demanding its lion's share of victims with each recurring year, while the other diseases are rare visitors.

The total number of deaths in the United States each year from tuberculosis is estimated at 150,000, which means a money loss of $330,000,000 to the country. This should be sufficient reason for preventive measures on our part, even if we leave out of consideration the distress of the victims and their families. The slow and insidious onset of tuberculosis no doubt tends to lessen the fear we have of it, but in this very fact lies much danger, since it is more difficult to persuade persons of the relation of cause and effect than in a malady where exposure is promptly followed by attack. The magnitude of the task deters some from undertaking it. Very recently a legislator who was being urged to assist in procuring State aid brought up such an objection. The task is unquestionably a great one, but it can be stated with assurance that the total eradication of tuberculosis is feasible.

Another class of obstructionists are those persons who regard all discoveries as "new-fangled notions," and quote the mode of life of their grandfathers, who did not find measures for the prevention of tuberculosis necessary. It is impossible to argue with such persons, as a rule. It may be pointed out, however, that tuberculosis is essentially a disease spread by overcrowding, and, like all communicable diseases, the more dense the population the greater necessity for preventive measures. Conditions change materially with increase of population, and the new conditions must be met by new measures which may have been unnecessary before.

Prevention of Tuberculosis.

All efforts at the eradication of tuberculosis to be successful must be based on the fundamental fact of its communicability, and in the main it is to be treated as the other contagious diseases, though

the restrictions need not be so severe, since more or less prolonged exposure is necessary to bring about infection.

Two parties are to be considered, the tuberculous person and the community, and while the former is entitled to every consideration and attention, the good of society in general must be the principal consideration which guides our actions. Fortunately, the interests of the two parties are not irreconcilable, and much can be done by education to smooth the difficulties which lie in our path. With this end in view there should be in every State, and in all large cities, societies whose object is the study of methods of prevention, and the dissemination of such knowledge in short, plainly written tracts among the people. In addition to this, Boards of Health should issue circulars constantly giving such information and advice. At present only twenty-two States and seven cities issue such circulars and recommendations, while five States have State societies and five cities have local societies for the prevention of tuberculosis. These societies can do much good also by shaping legislation. States and cities should have uniform laws regarding expectoration in public conveyances, buildings and on sidewalks, overcrowding of factories and tenement-houses, the construction of such buildings as regards light and ventilation, and the employment of children under age. Health officers should have the power to force ignorant and vicious tubercular persons who persist in reckless expectoration and other offenses against public hygiene into hospitals provided for them by the public. There should be notification and registration of the persons suffering from phthisis, and apartments occupied by such persons should be thoroughly disinfected periodically, and always after death or vacation of the premises before new tenants are allowed to enter them.

All of these things can be carried out with little or no increase of expense, and much good can be accomplished along these lines. However, the urgent need is for institutions in which the sick can be cared for and instructed. These should be of at least two types :— sanatoria, built in open country districts in regions known to be specially adapted to the treatment of tuberculosis; and, second, hospitals in or near cities for the hopelessly ill and destitute, where the maximum of comfort can be given them, and where they will cease to be sources of infection to their families and the public in general. In connection with the sanatoria convalescent farms are most useful, and may be made self-sustaining to a certain extent. On

such farms patients who are well enough to be discharged from the sanatoria can find light employment under good conditions until strong enough to return to their usual avocations in factories, etc., without danger of relapse.

I have not tried to outline an ideal method of dealing with tuberculosis, and much could be added to what has been said, but have limited myself to what appears to me imperatively demanded by the conditions which confront us, and to what is entirely in our power to effect. The affair is, however, beyond private charity, and governmental aid is necessary, each State doing its share.

In spite of the enormous expenditure which would be involved in providing hospital accommodations for the indigent tuberculous, it would cost less than the present money loss to the country from deaths alone, estimated, as said before, at $330,000,000 annually; and in a few years we could confidently expect a marked and progressive decrease in outlay. It must be borne in mind that the demonstration of the communicability of tuberculosis has resulted in special hardships to the poor consumptive, since most general hospitals now close their doors to these afflicted ones. The poor consumptive reaps but little aid from the vast donations from public and private sources to general hospitals; hence the urgent necessity for special provision for them, both on the score of humanity as well as protection to the public health.

Hand in hand with such measures should go efforts directed to the eradication of tuberculosis from cattle, since we must look on cattle as the source from which a certain amount of human tuberculosis springs, chiefly in children.

Without entering into matters of controversy, the following proven facts may be stated as grounds for this belief:

(1) The tubercle bacillus as found in bovine animals differs from that found in man chiefly in its greater virulence for practically all experimental animals, including man's nearest relative, the monkey. It would be an anomaly if man, one of the most susceptible of all animals to tuberculosis, were immune to the most powerful type of the germ of tuberculosis known to us.

(2) There are numerous well-authenticated cases of accidental inoculation of man by the bovine tubercle bacillus, with the production of typical disease at the point of inoculation. Some of these cases have been followed by general tuberculosis, ending in death, attributable with good reason to the inoculation.

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(3) A number of instances have been recorded in which the onset of tuberculosis followed the use of milk from tuberculous cows. In some of these the relation of cause and effect is so close that Nocard has well said "they have almost the value of an experiment."

(4) That food containing bovine tubercle bacilli may and does produce tuberculosis in man seems already proven by finding in the intestinal tract (mesenteric glands) of children who have died of tuberculosis tubercle bacilli which have all the characteristics of the bovine germ, and which have an intense degree of virulence for cattle.

(5) The close relationship of the human and bovine tubercle bacilli has been shown by the recent experiments in immunization, in which it has been proven that injections of bacilli from human sources will protect animals against virulent bovine germs. This has been done by Trudeau, De Schweinitz, and Pearson and Gilliland in this country, and by Behring and Thomassen in Europe.

What Has Been Done in the United States Toward the Suppression of Tuberculosis.

Three States and four cities require the reporting of cases of tuberculosis; in five States and five cities report is optional; in one city it is under litigation.

Two States have general anti-spitting laws, while five States have local laws, and fourteen cities have their own laws. Twenty-two States and seven cities issue circulars and recommendations.

The United States Government has two sanatoria for persons in its employ; five States have five special institutions, and nine States have projected sanatoria. Two States have tent colonies on a small scale. Only three cities have special municipal hospitals for consumptives. There are forty-two private institutions in eleven States, some supported by private charity, some partially self-supporting, and some for pay patients only.

Twenty States and twelve cities have laws regarding bovine tuberculosis. Twenty States have done nothing in regard to human or bovine tuberculosis; six States have done something to combat tuberculosis in man only, and eight States have done something against bovine tuberculosis alone.[1]

[1] These figures have been taken almost entirely from the valuable paper of Dr. S. A. Knopf, read before the American Medical Association at Saratoga, June, 1902. Since then considerable advance has been made.

Comparisons are said to be odious, but in the hope of stirring up our people in the United States, I quote the most recent statistics of what is being done in Germany, which may be taken as an index of the attitude of most of the countries of Europe toward the scourge of tuberculosis.

The Fight Against Tuberculosis in Germany.

According to the Imperial Health Office in Berlin, the deaths from tuberculosis are about one-tenth of those of all diseases. In 1899 the number of patients treated in hospitals in the empire was 226,000. According to the latest statistics there are at present 57 public sanatoriums for the tuberculous in Germany, of which 34 are located in Prussia, 6 in Bavaria, 2 in Saxony, 1 in Wurtemberg, 1 in Hessen, 1 in Sachsen-Weimar, 1 in Thuringia, 1 in Reichsland, 3 in Baden, 2 in Brunswick and 5 in the Hansa cities. Besides these there are 4 four institutions near the sea—namely, Nordeney, Wyk, Gross-Muritz, Zoppot. There are also 23 public sanatoriums nearly completed, among these being Buch, near Berlin. The city of Berlin has at the present time 3 public sanatoriums—namely, Malchow, Blankenfelde, Gutergotz. There are also 20 private German sanatoriums, and 1 in Davos (Switzerland). In the 78 sanatoriums for the tuberculous there are 7000 beds. If we calculate that each bed is used by four persons in the course of a year, we find that about 30,000 tuberculous patients annually enjoy the benefit of treatment in the sanatoriums. The efforts made in the German Empire to combat tuberculosis, both by direct regulations and by general preventive measures, are being actively carried on. In particular, the Imperial Government, the governments of the different States, the executive authorities, the national insurance institutions and the municipal governments are seriously and actively participating in this work. The result of these efforts, which have been now carried on for some years, is already noticeable in a decrease in the number of deaths from tuberculosis, which in the future will be still more marked (*American Medicine*, March 21, 1903).

Artificial Immunity and Serum-Therapy.

For many years constant effort has been made to discover a serum or lymph for the specific treatment of tuberculosis, and several such substances have been announced from time to time. All of them

have proved disappointing, however, not excepting Koch's lymph or tuberculin, the discovery of which was hailed with delight and enthusiasm by physicians and consumptives alike in all parts of the world. Recently it has been demonstrated authoritatively that it is perfectly possible to produce artificial immunity against tuberculosis in animals by a process of vaccination, as such methods are now generally termed, and with this demonstration comes the well-founded hope that we are within sight of the goal so much hoped for, the discovery of a specific serum for the cure and prevention of tuberculosis. Indeed, we have already the news that two well-known bacteriologists have produced such a substance. While the details have not yet been made public, the names of these two men, Behring, of Germany, the discoverer of diphtheria antitoxin, and Marmorek, of the Pasteur Institute, in Paris, the discoverer of streptococcus antitoxin, are of such weight as to justify strong hope that they have achieved success. We may feel assured that marked progress has been made, to say the least.

I have not dwelt on the pathetic side of this question—the fearful loss of life and suffering entailed by a preventable disease. On this point I cannot do better than to quote a short editorial from a recent issue of *American Medicine* (March 28, 1903). While this deals with the city of New York, it is equally applicable to every city in the United States, the figures only needing modification.

The Tragedy of the Homeless and Friendless.

"In the year 1902, in the borough of Manhattan, there died of tuberculosis, chiefly in the various hospitals of the city, 1787 patients. Of these, 950 were "not known" at the addresses given; 456 gave no addresses; 275 gave the address of a lodging-house, and 106 gave an address outside of the city. It must be remembered that these deaths constituted only about one-seventh of all the deaths that took place. Moreover, for every death there are, according to Dr. Farr, about two years of illness endured. When one thinks how much our happiness, even in health, depends upon home and love and friendship, and that in illness and death the blessedness of these things is vastly increased, and then when one realizes that there are so many thousands of the sick and dying in our cities utterly homeless and friendless, the pity of it all becomes indeed terrible. The tragedy of obviable disease and needless

death kindles our zeal to stop the spread of infection, to discover the means of preventing the suffering, and, when this is not possible, to surround the lonely sick and dying with the best medical skill, attention and kindness that is possible. The desolation of their appalling loneliness is often doubtless greater than that of their illness and oncoming death combined."

PHILADELPHIA, April 4, 1903.

ON ARTIFICIAL PRODUCTION OF CRYSTALLIZED DOMEYKITE, ALGODONITE, ARGENTODOMEYKITE AND STIBIODOMEYKITE.

(Plate V.)

BY GEORGE A. KOENIG.

(*Received June 1, 1903.*)

In a paper on mohawkite, domeykite and other copper arsenides of the Mohawk mine (*Zeitsch. f. Krystall.*, etc., Vol. xxxiv, 1 Heft), I mentioned some attempts made by me to obtain domeykite in measurable crystals by the action of arsenic vapors upon metallic copper. One experiment gave crystals, although not measurable, but further trials failed at the time, evidently through my inability to maintain the proper temperature by means of an Erlenmeyer combustion furnace. The range between the temperature at which the crystals form and that at which the crystals melt is a very narrow one. On the other hand the eagerness with which the copper absorbs the arsenic causes heat, and hence the difficulty in adding just the right quantity of thermal energy from the outside. It occurred to me to try an electric current as a source of heat. The very first trial gave most promising results. The experiments were taken up in November, 1900, and continued until March, 1901. The adjoining figure illustrates the simple apparatus which proved itself adequate to all requirements.

In watching the rapid growth of the crystals the similarity of the phenomenon with the development of an egg occurred to me, and I applied the name "incubator" to the apparatus, than which no other could be more expressive.

The incubator consists of a piece of combustion tubing (T), closed at one end. The length is unimportant since only about three inches of it are in actual use. I have varied the diameter from three-eighths to three-fourths inch with no apparent difference in the action. The crystals do not grow any larger in a large tube than in a small one. Around the tube is wound a very thin platinum wire (W), beginning at the closed end. In order to keep the coils separated I laid three strips of thin asbestos paper (E) lengthwise upon the glass and then began winding. The first turn returns to the start, a twist is made, and thus a well-fixed start is secured

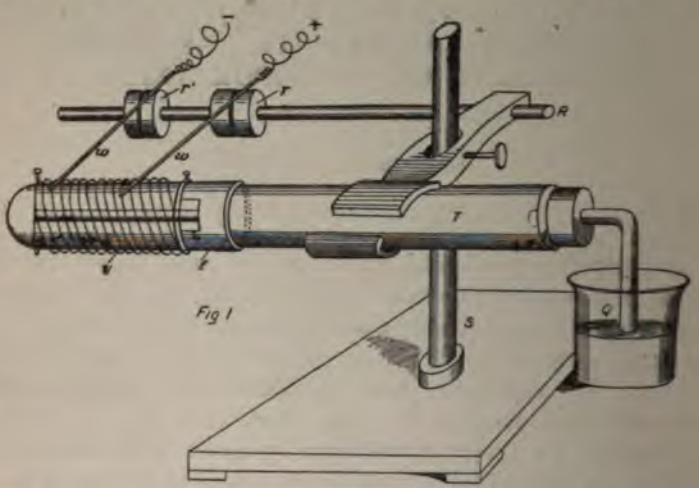


Fig 1

which will prevent the wire from slipping. The pitch of the thread will be governed by the maximum of heat desired. This will be variable with different metals and may be varied even for the same metal, as I have frequently done, the variation being between one-eighth and one-thirty-second of an inch. The last coil is secured in the same way as the first. Two inches of winding were mostly sufficient. Whenever the glass gets to full red heat the wire will fuse into it and will be broken in unwinding. To avoid this spoiling of the wire it would be the best thing to cover the whole glass surface with the asbestos sheet. But doing so would also prevent the observation of the phenomena occurring within. One might as well, or even preferably, use a porcelain tube. One would have to forego the great pleasure of seeing the so-called inanimate things

come to life, and one would make many more failures by either too much or too little heat. The wear and tear of the wire seems trifling when held against this loss. Being thus prepared the tube is ready to be charged.

At A (Fig. 2) I place from five to ten grams of resublimed arsenic, on top of this a loose plug of asbestos (P). In the first experiments I thought copper filings would be the best material to



Fig 2.

act upon. These filings I poured on top of P, forming a column about one inch high, and secured this column by means of a second asbestos plug (P^1). Such an arrangement of parts promised to restrain the arsenic vapors from passing by the copper without action. It proved an unnecessary precaution, as the copper acts toward that vapor as a sponge toward water. Coarse turnings were tried instead of filings, and later solid copper bars with even better results than the filings had given. Similarly the close proximity of the copper to the plug P was found objectionable and, therefore, in all the later experiments the tube was placed, after charging the arsenic and inserting P, in a horizontal position by means of a clamp at the open end. Then the metal pieces to be acted upon were shoved into the desired distance from the plug, a loose asbestos plug next to the metal to avoid air currents, and finally a stopper holding a narrow glass tube, bent at right angles, was inserted into the open end. The glass tube was then made to dip under mercury and thus expansion of the air made possible, without danger of air entering. Whatever oxygen was in the tube made As_2O_3, which was always found as a ring sublimate behind the metal. Fig. 1 shows the outside of the tube, clamped to the stand S. The stout contact wires, w, w^1, were found very serviceable. By their use the field of high temperature may be enlarged or restricted as well as shifted. These wires are simply laid upon the coil wire. Their position must not be shifted or altered without switching off the current; the thin wire will melt in the moment when the contact is broken. I spoiled considerable wire in this way, besides the time consumed in rewinding is quite an item. A suitable, easily changeable resistance to modify the tone of the

thermic energy is made part of the circuit. A water resistance answers very well if a Mariotte's bottle be provided to keep the water level, and if the one wire be fastened to a swivel; the latter arrangement permits a quick and easy modification to $\frac{1}{1000}$ amps. In my work with the incubator a drum resistance coil with roller contact was used. The apparatus was placed into a dark room in which the faintest glow could be seen and thus the lower limit of temperature was probably 450° C., with the upper limit of C. 500° to 700°. The most satisfactory range for domeykite is about 600° C.

Aims of the Investigation.

At the start the aim was not so much the mere production of domeykite crystals, as the demonstration that nickel and cobalt might replace copper in the molecule without changing the symmetry, in other words to establish the isomorphous character of domeykite and mohawkite. This original scope became at once wider, when the results showed the ease with which domeykite was formed in good crystals. The action of arsenic upon iron, lead, silver, cobalt, nickel was included and equally satisfactory results were fondly hoped for.

A still farther circle could be described by drawing in antimony since silver was known to unite with antimony as Ag_3S_4. The hopes were not realized. Under other conditions perhaps better results may be obtained, at least in some cases. I am referring here to the action in vacuo. Up to this time I have not tried the vacuum, so much other work is constantly crowding in. I will not pre-empt work in this line and shall gladly see any colleague step in to take up this undoubtedly highly interesting work.

1. Action of Arsenic Vapors upon Copper in the Incubator —Domeykite.

a. Coarse copper turnings were placed in the tube (C. Fig. 2) so that about three-fourths inch of free space were left between the copper and the asbestos plug P, and the contact wires were so placed that the evaporation of the arsenic was fairly rapid, whilst the temperature of copper remained near the lower limit of say 500° C. Soon one saw shooting out from the copper very thin, brilliant leaves. The direction of growth was parallel with the tube's axis. The growth keeps up until the entire free space is filled with the bright crystal aggregate. The latter looks much like sublimed arsenic, and that I

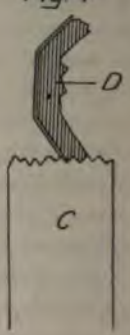
thought it to be until the analysis showed it to contain 72.9 per cent. of copper. The crystals even penetrated into the asbestos, and from this very extremity the material for the analysis was taken. This experiment was carried on from 8 A.M., January 11, 1900, for forty hours. Here was a phenomenon of molecular or ionic activity without parallel; at least to me extraordinary, for I had not seen any record of a similar observation. It is not difficult to understand the building up of crystals from a medium which contains the molecules in the liquid or gaseous state. But what I observed here implied a very different condition of things. Not even the skyward growth of a tree, which somewhat resembles this stretching out of the domeykite toward the supply of arsenic, is comparable. For the cells draw their nourishment from the liquid sap and the gaseous air. The growing may happen from the root by pushing or by growing at the front. In the latter case the copper ions must be supposed to be going like the ions under the direction of a current, but going in the solid condition, and this is the point at which the imagination recoils. Either alternative rests upon a push or a draw impulse. The present experiment would seem to point toward a push from the root as the cause, that is to say to a mobility of the copper arsenide molecule Cu_3As. In Fig. 3 is represented one of the results of a later experiment, which gives support to the notion that the copper ions are moving and not the molecule Cu_3As. Here C is a piece of copper turning. On a slender stylus S sits the large domeykite crystal D (the tabular type, three millim. in diameter). The crystal is incomplete on one side. From it leads a second stylus S^1, and upon this another somewhat incomplete crystal of domeykite D has been growing. All the material for the crystals must have come through the stylus S. Instead of all the *material*, I should say more correctly all the copper. The stylus habit for the crystallization is very common; Fig. 3 merely represents an unusually fine specimen of this habit. Looking at the phenomenon of molecular mobility in the solid state merely as a physico-chemical process, aside from crystallization, I can see an analogous occurrence in the so-called cementation process of steel or case-hardening process. In this process a bar of soft iron is exposed to red heat in a packing of solid charcoal, and becomes gradually converted into carbid Fe_4C to the very innermost parts. It would seem that the solid carbon ions become mobilized, passing from one group of iron ions to the next until

chemical saturation has been reached. To my knowledge, however, no experiments have been put on record which absolutely precluded the coaction of gasified carbon as CO; I mean that the experiments were not carried on in perfectly air-tight vessels. Yet, granted even that the solid carbon travels by exchange through a bar of iron, the phenomenon is not quite correlated to our problem. For if the two were similar then the arsenic would have to penetrate to the core of the copper chip without altering practically its original shape. But in the specimen Fig. 3 the copper chip C is perfectly bright metallic copper, even immediately under the stylus S. Furthermore all the other metals behaved toward arsenic vapors as iron toward carbon: the arsenic penetrates



Fig 3

Fig. 4



and crystals do not shoot forth. Copper possesses, therefore, a *unique ionic mobility*. Since copper stands at the head as a conductor of both heat and electricity, may not this be due to that mobility of the ions?

b. If a copper chip be placed into the incubator and both resistance and contact wires be so adjusted that very little arsenic volatilizes, and that the copper is just below glowing heat, that is dark in a perfectly dark room, then the domeykite crystals arise from the copper as very thin tabular individuals, often of perfect hexagonal outline. Many of the crystals are only fractional (Fig. 4), and in this case look like bristles or spines, always at right angles to the surface, or if the latter be curved then the bristles will be in radial position. At first a few scattered crystals will come out, always nearest to the supply of arsenic, but later the entire surface will become covered with bristles. Under these conditions large

and full-faced crystals were never obtained. The largest crystals of the tabular habitus 4–5 millim. with prismatic and pyramidal faces well developed, but striated, so that they did not serve for measurements, were grown in a twenty-four-hour experiment. The crystals are fast to the asbestos of the plug P.

c. Experiment made January 14, 1901.—A piece of one-fourth inch copper wire two and one-half inches long was wrapped at one end with asbestos cord so as to form a plug which would support the wire within the incubator in a central position, thus giving a chance for free growth in all directions. At the end of fourteen hours the wire had been modified as shown in Fig. 5. The result was unexpected, probably owing to the change of current in the early morning, when the dynamo current had replaced the storage battery and the temperature had risen beyond the intended point.

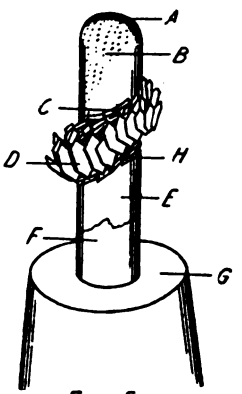


Fig 5

It is, however, all the more instructive, although a failure of the intention. Exceptionally large and fine crystals were expected to form by the arrangement, hence the failure. Instead seven distinct zones appeared, each telling a different story. Zone 1st (*a*). The end of the wire nearest to the asbestos plug and the arsenic and in the centre of the heated field is completely fused, showing a lead-gray color and dull compared to its neighbor. The end is deeply converted into arsenide and this has been fused. Zone 2d (*b*). Bright gray of the color of antimony, a jumble of crystalline faces, but no crystals, has been partly fused. Zone 3d (*c*). A narrow strip of small but well-formed crystals, which belong to

thick tabular type, has not been fused. Zone 4th (*d*). A collar of bristling hexagonal plates, some with prismatic and pyramidal faces, but withal belonging to the thin type. This collar of crystals looks jet-black, the contrast with the bright gray both striking and beautiful. In different light the crystals always appear black, only in reflected light the color is gray. This zone marks the minimum of temperature at which combination of copper and arsenic takes place. Zone 5th (*h*). Is very narrow and dull gray; it reveals miniature crystals of the thin type. Zone 6th (*l*). Shows the beautiful pale red color of pure copper. Evidently arsenic vapors surrounded this part of the wire; the temperature sufficed to let this arsenic combine with the oxygen of the surface, and thus give the latter the pure copper color, the peroxyd subliming. Next to this we find the wire with the usual red color due to a thin film of cuprous oxide.

d. A piece of quartz two inches long and just wide enough to go into my largest combustion tube, that is three-fourths inch, had a number of native copper crystals, pseudomorphous after quartz. This specimen was incubated. A growth of thick tabular domeykite crystals formed all over the one side of the quartz. The artificial nature is disguised by the quartz and the epidote in the association to such an extent that any mineralogist would take it as a thoroughly natural production and hence a most unique specimen.

2. Action of Arsenic Vapors upon Alloys of Copper, Nickel and Cobalt—Mohawkite, Keweenawite.

An alloy was made of the three metals in about the same proportion in which they are found in the natural mohawkite, that is

Cu	= 74
Ni	= 21
Co	= 5
	100

The alloy was cast into a bar one-fourth inch wide, and parts of this bar were successively exposed in the incubator under different conditions of temperature, of rapid or slow evaporation of arsenic.

First Experiment, December 18, 1900.—The alloy is converted into filings and these are put directly against the plug P in the

manner as described under copper, but the filings only occupied the lower half of the tube. Upon the upper flat surface crystals form of the thick tabular type, the first pyramid prevailing over the basal plane. The crystals are coherent laterally, crustlike, over a loose aggregate of bright, light gray crystalline matter with indistinct faces. At the time I thought these two materials were alike. But recently, on re-examination, it is seen that whilst the crystals have become much tarnished, the gray material has not changed at all. The crystal layer was detached as much as possible from the loose substance, for the analysis, but it was not possible to do this thoroughly.

The analysis of the crystals gave (0.216 gram):

Cu = 66.37	: 63	= 1.0524	1.0939
(Ni + Co) = 2.43	: 58.6	= 0.0415	
As = 30.90	: 75	=	0.4120
99.70			

Ratio:

$$(CuNi\ Co) : As = 2.655 : 1.000$$

The analysis of the gray loose material gave (0.2325 gram):

Cu = 44.30	: 63	= 0.7032	
Ni = 12.54	: 58.6	= 0.2822	0.9854
Co = 4.00			
As = 39.25	: 75	=	0.5230
100.09			

Ratio:

$$(CuNi\ Co) : As = 1.88 : 1.00 = 2 : 1$$

This then is typical Keweenawite, described recently by me (*Amer. Journ. Sci.*, Vol. xiv, December, 1902) as found at the Mohawk mine. The non-tarnishing quality is inherent also in the natural mineral, as mentioned *l. c.* The crystals on the other hand are mohawkite; the excess of arsenic making the ratio 2.655 : 1 instead of 3 : 1 is explained by the impossibility of separating the crystals from the adhering keweenawite.

Experiment of December 24, 1901.—Instead of filings, two fragments of the alloy were exposed in the incubator for twenty-seven hours. Hexagonal plates, very thin, formed upon a crust of gray material strongly crystalline. The plates stood at right angles to the surface and could be brushed off with small camel's-hair brush.

The analysis with 0.0867 gram of the absolutely pure crystals gave :

$$\left.\begin{array}{rl} Cu = 69.31 : 63 & = 1.1002 \\ (Ni + Co) = 2.70 : 58.6 & = 0.0461 \end{array}\right\} 1.1463$$
$$As = 28.12 : 75 = 0.3750$$
$$100.13$$

Ratio :

$$(CuNi\ Co) : As = 3.06 : 1.00 = \text{mohawkite}$$

Both experiments show conclusively that nickel and cobalt will enter the crystals without changing the hexagonal symmetry; that domeykite and mohawkite are indeed isomorphous. At the same time the interesting fact is to be dealt with that nickel and cobalt do not pass into the arsenide with the copper in the ratio in which the alloy exposes them to the action of the arsenic vapors. That in fact the ionic mobility of nickel and cobalt is only approximately one-sixth that of the copper. For in the alloy the ratio of copper to nickel and cobalt is nearly 4 : 1, whilst in the crystals it is 25 : 1. The highest percentage of Ni + Co furnished for mohawkite was 4.51, but the analysis was otherwise unsatisfactory.

3. Action of Arsenic Vapors upon Nickel.

Two cakes of nickel were exposed in the incubator for twenty-four hours. No crystals could be obtained, not even of the most imperfect type. A brittle material formed as a thin crust of a dull gray color. It was not analyzed. The action upon cobalt was similar. The ionic mobility of these metals under these conditions seems to be near zero. We may infer that in the previous experiments Ni and Co were moved by infection from the copper's ionic vigor.

4. Action of Arsenic Vapors upon an Alloy of Copper with Silver—Argentodomeykite.

The metals were melted together in the proportion 9 : 1. The alloy was cast into a bar and fragments of this were exposed in the incubator.

Experiment of January 22, 23, 1901.—The material for action is a solid piece of the bar about 15 x 25 millimeters. The crystals grew out of this alloy towards the arsenic as rapidly as out of pure copper. They are of the tabular variety, medium thickness. The

pyramidal faces are hollow (see Dr. Wright's Fig. 3). The dark gray crystals are surrounded at the base by a fringe of *silver-white crystals* of the thin plate type. It happened that the exposure began about 9 A.M. on a Saturday morning. At 6 P.M. the crop of crystals had developed finely, but I hoped that they would become extra large by longer exposure. On Sunday I was prevented from going to the laboratory, and on my arrival on Monday I found the incubator barely warm to the touch. The storage battery had run down over Sunday, and to this accident we owe this beautiful and interesting preparation which I now hold in my hand. On seeing the silver-white crystals I thought, first thing, that I was beholding a silver arsenide, but the analysis proved my judgment to have been in error.

The composition is

$$\left.\begin{array}{l} Cu = 80.49 : 63 = 1.2773 \\ Ag = 2.60 : 107.6 = 0.0242 \end{array}\right\} 1.3015$$
$$As = 16.93 : 75 = 0.2257$$

Hence the ratio:

$$(CuAg) : As = 5.77 : 1 = 6 : 1$$

This substance then is *argentoalgodonite.*

The dark gray crystals have the composition:

	Cu = 70.40
	Ag = 2.30
(By difference)	As = 27.30
	100.00

This is the ratio:

$$(CuAg) : As = 3 : 1$$

or what I will name *argentodomeykite,* which we shall, sooner or later, find undoubtedly as a natural mineral. But how about the algodonite? In no other experiment was it observed. Since the form of the crystals is identical with the argentodomeykite, I venture to assert that the algodonite is pseudomorphous after the domeykite, and owes its existence to a retrogressive process in this way: when the temperature was slowly going down (with the current from the battery) the arsenical atmosphere became more and more rarefied with the greed of the metallic copper still active. Hence the copper began to draw the arsenic from the nearest

domeykite crystals and the latter became algodonite. Since the algodonite is only found at the base, near the copper, the explanation seems to me plausible enough.

Experiment of January 5, 6, 1901.—A piece of silver was exposed in the incubator. It was supposed to be quite pure; but, as will be seen from the analysis, it contained several per cent. of copper. For several hours no action appeared to take place, behavior being similar to nickel. Then the edges began to round and towards evening the piece of alloy went into complete fusion at a temperature certainly not above 450° C. Seen by candle-light, through the glass tube, the material had the appearance of a large drop of mercury, being seemingly very mobile. The following morning (with the weaker current) it was found solidified, but no sign of crystals. The substance broke readily under the hammer; the fracture shows cleavage faces and a light gray color.

The analysis gave (0.4795 gram):

		Atoms.	
Ag =	74.32	0.688	0.763
Cu =	4.72	0.075	
(Difference) As =	20.96		0.273
	100.00		

Ratio:

$$(AgCu) : As = 2.79 : 1.00$$

There is, therefore, a molecule Ag_3As with a tendency, however, to pass into Ag_4As; some of the latter is shown in the ratio $\frac{2}{3}$, instead of 3, which corresponds exactly to 4 Ag_3As + Ag_4As.

Experiment of January 21, 1901.—Piece of alloy (1 copper, 1 silver) exposed twenty-one hours. A beautiful growth of thick tabular crystals, which sit up on a gray crystalline layer, under which appears a thin zone, silver-white in color, 1/2 millimeters thick; then comes copper-red. The growth is entirely in the axis of the piece and tube towards the arsenic.

The analysis of the crystals gave:

Cu =	62.02	0.9844	1.0882
Ag =	11.21	0.1038	
As =	26.77		0.3569
	100.00		

Ratio:

$$(CuAg) : As = 3.05 : 1.00$$

The silver-white zone under the crystals demonstrates to the eye the difference in the ionic mobility of copper and silver. One sees how the copper is drawn away from the silver. It would be of interest to know whether the outermost crystals carry less silver than those nearest the metallic base, but as this gain of knowledge would also involve a destruction of the specimen I abstained, satisfied with the average result as exhibited in the above analysis.

Experiment of February 24, 1901.—An alloy of 1 copper with 1 silver was made and a piece weighing about 5 grams was exposed in the incubator for fourteen hours (over night). The front and upper surface of the ingot was found covered with crystals. They are not good, but they show distinctly the habitus of the thick tabular domeykite. There is no tendency to rise; the silver is evidently as little mobile as the nickel and acts depressingly upon the activity of the copper. The crystals are laterally grown together, forming a strongly cohering crust, which cracks off with the hammer blow.

Analysis of the crust gave (0.3057 gram):

		Atoms.	
	Cu = 55.87 = 0.887		1.026
	Ag = 15.01 = 0.139		
(By difference)	As = 29.12 =		0.388
	100.00		

Ratio:

$$(CuAg) : As = 2.65 : 1.00$$

This corresponds to a mixture of 5 $(CuAg)_2As$ with 3 $(CuAg)_2As$. The tendency is always rather for the building up of Cu_3As than of Cu_2.

Experiment of February 28, 1901.—A piece of alloy 1 Cu + 1 Ag exposed in incubator for two days and nights at very low temperature. The end reaching towards the arsenic showed a fused, bluish-gray, apparently homogeneous material. At the opposite end are small crystals with bright faces. Habitus: steep hexagonal pyramid with striated sides, capped by the normal or fundamental pyramid.

Analysis with 0.3412 gram gave:

		Atoms.	
	Cu = 40.94 = 0.650		0.989
	4 Ag = 36.62 = 0.339		
(By difference)	As = 22.44 =		0.2992
	100.00		

Ratio:

$$(CuAg) : As = 3.3 : 1.00$$

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This is the single instance in which the metal exceeds the $\frac{3}{1}$ ratio, if we exclude the instance of the algodonite. I venture to explain it by a similar process of retrogression, or, perhaps, better, by ingression of the still very mobile metal ions into the molecule $\frac{3}{1}$ after the arsenic vapor had become too rarefied for addition from the outside, as the temperature was sinking—*i.e.*, the current going down. All the experiments with the copper-silver alloy prove: 1. Silver and copper together replace one another isomorphously in the molecule $\frac{3}{1}$. 2. The representation is 63 Cu by 107.6 Ag—*i.e.*, divalent copper with monovalent silver. 3. There is a molecule, Ag_3As, whose melting-point is below the temperature of formation; hence not crystallizable in the incubator. 4. Higher temperature tends to forming Ag_3As, same as Cu_3As.

5. The Action of Arsenic Vapors upon an Alloy of Copper and Antimony—Stibiodomeykite.

If copper and antimony were melted together in such proportion as Cu_6Sb, which corresponds nearly to copper 3 parts, antimony 1 part, in percentage Cu = 75.6, Sb = 24.4, and such an alloy were to be exposed at the proper temperature to the arsenic vapors, one might be justified in supposing that by simple addition of one atom of As we would get

$$Cu_6Sb + As = 2\,Cu_3(SbAs)$$

if there were a tendency in the elements to form such a combination; or if the molecule Cu_3Sb(Cu = 66 Sb = 34) were exposed, then one As might couple together two molecules of Cu_3Sb.

Experiment of February 1, 2, 3, 1901.—The alloy Cu_6Sb was exposed for three days and nights at very low temperature. In the absolutely dark room the wire coil showed dull redness. Very large tabular crystals form, drawn in the axis of the tube forward the arsenic. The crystals have the color and habitus of domeykite. Their composition is:

	Cu =	69.34
(0.1422 gram)	Sb =	1.26
(By difference)	As =	29.40
		100.00

Ratio:

$$Cu : (AsSb) = 2.76 : 1.00$$

The analysis reveals two points: 1. Arsenic does not add itself to a ready-formed molecule Cu_3Sb. The same preferential attraction towards copper comes into play as in the case of the metallic constituents of copper alloys. The ionic mobility of antimony is low; at any rate, at the low temperature in use during this experiment. 2. The ratio indicates that antimony is probably merely mechanically carried along so long as copper is at hand for the arsenic; for if antimony be considered out of the molecule the ratio will be Cu : As = 2.82 : 1.00. But even this is unsatisfactory for a crystallized body with no mechanical admixture likely, for the crystals were all separate and large enough to be fully scrutinized.

For more light I went to examine into the material directly under and back of the large crystals. This material is loose, in small loose grains of angular habitus, not scaly at all, as the large crystals. Habitus quite unlike that of the domeykite; color darker gray. The analysis of this material (0.0570 gram) gives:

$CuO = 0.0393$	$Cu = 55.44$	: $63 =$		0.08800
$Sb_2S_3 = 0.0028$	$Sb = 3.51$	: $122 = 0.0287$	}	0.571
$As_2S_5 = 0.0480$	$As = 40.66$	: $75 = 0.5421$	}	
	99.61			

Ratio:

$$Cu : (AsSb) = 880 : 571 = 3.08 : 2$$

As the copper becomes scarce, the arsenic being still plentiful, this new 3/2 molecule forms. In spite of the superabundance of antimony, the selection of copper continues. With nickel we saw in similar conditions the forming of 2/1 molecule. The affinity for antimony is quite low, and yet it is probably the influence of the latter through which 3/2 and not 2/1 are brought into being. The excess of arsenic in the large crystals accounts for itself by the presence of this 3/2 molecule under the influence of antimony.

Experiment of February 4, 1901.—The same alloy Cu_3Sb was exposed for thirty-six hours at a higher temperature, about 550° C. Two products were obtained. Forward, towards the arsenic, a lustrous gray mass, apparently of fused crystals. The outermost part of this mass was broken off, revealing a hollow center, an inner layer of dark-gray mass, an outer layer of lighter color. Could not separate the two. Let this be material (a'). The second substance

is composed of crystals of the domeykite habitus, thin and thick tabular; all but very few show rounded edges—that is, incipient fusion. A part of the crystals was removed. Material (*a*).

Analysis of *a* (0.4801 gram); many individual plates, others grown together.

Cu = 69.79 :	63 =		0.1078
As = 20.32 :	75 =	0.2709	0.3509
Sb = 9.74 :	122 =	0.0800	
99.85			

Ratio:

$$Cu : (AsSb) = 3.11 : 1.00$$

These crystals are thus proved to be *stibiodomeykite*. It also follows that higher temperature increases the ionic mobility of the antimony.

The analysis of the material (*a'*) (0.4823 gram) gives:

Cu = 45.10 :	63 =		0.7158
Sb = 36.83 :	122 =	0.3019	0.5428
As = 18.07 :	75 =	0.2409	
100.00			

Ratio:

$$Cu : (SbAs) = 1.32 : 1.00 = 4 : 3$$

I take this to mean a mixture in which the outer crust is Cu(SbAs), whilst the inner layer is $Cu_4(SbAs)_2$; $1/1 + 3/2 = 4/3$.

Experiment of February 7.—A fragment of the alloy Cu_4Sb was again exposed with the intention of keeping the temperature, if possible, below that of the one preceding, and yet higher than in the first experiment. The exposure was forty hours. It must be remembered that owing to the local conditions a perfectly uniform temperature could not be maintained unless the ammeter were under steady observation—an impossible or at any rate too difficult a proposition. Hence the temperature would steadily decrease, whilst the potential sank from 120 to 110 volts and would rise (during the night) to 125 volts, when in the forenoon the dynamo was coupled to the storage battery.

The original alloy was found covered with three products:

1. A fine granular crust.
2. Over this a peculiar sort of crystals not showing any one of

the habitus of domeykite. These crystals are all fused together laterally; the faces are rough like the leaves of reeds. They are not measurable.

3. On these faces rise curious pyramidal forms as shown in Fig. 6. The crystals are slender and small. Their faces are bent and very uneven from the alternation of pyramid and prism. The acute pyramid predominates, but apex is closed by the normal pyramid

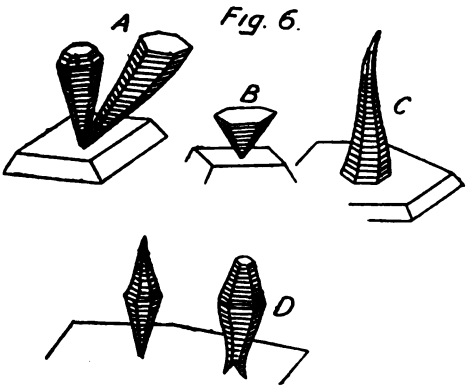


Fig. 6.

(see Dr. Wright). Some of the forms, as at *a.a.*, resemble minute cup corals; others, as at *b.d.*, imitate a club, whilst *c.c.* may be likened to a church steeple.

ad. 1 and 2. Crust and crystals cannot be separated.

Analysis gives (0.0532 gram):

Cu	= 67.74 :	63	= 1.0752
Sb	= 1.00 :	122	= 0.0082
(Difference) As	= 31.26 :	75	= 0.4168
			100.00

Hence ratio:

$$Cu : (AsSb) = 2.52 : 1 = 5 : 2$$

Corresponding to a molecular mixture:

$$3'/1 + 2/1 = 5'/2$$

ad. 3. The crystals are so small that the whole of them would not make 0.1 gram. The sacrifice of any of them came hard. 3.97 milligrams weighed on a button balance, which is accurate to

0.005 mg., was dissolved in HNO_3 and evaporated to dryness. The dry mass dissolves in water, to which a drop of dilute H_2SO_4 has been added. Solution is without opalescence. Hence *antimony* can only be present in traces. Then 2.78 mg. of fine copper wire was dissolved in HNO_3, this being just seventy per cent. of the arsenide. Both solutions made ammoniacal were compared in proper dilution of 50 cc. on the colorimeter. The color of the solution from the pure copper is slightly lighter and by adjustment brings the percentage

$$Cu = 72$$

The crystals are thus proven to be *domeykite* and not stibiodomeykite. The strange habitus of the crystals, so unlike that of the prevalent habitus, must be looked for in the influence of the antimony, though the latter does not itself enter the composition.

6. Action of Arsenic Vapors upon Zinc.

Experiment of March 2, 1901.—A piece of chemically pure zinc, broken from a stick, was exposed for twenty hours. The surface is covered by a crust which is developed into mamillary groups, somewhat like psilomelane. The crust detaches itself by a blow from the hammer and breaks into flaky pieces resembling graphite.

Analysis gives (0.222 gram):

$$ZnO = 0.1720 = 0.1381\ Zn$$

Hence

$$Zn = 62.20$$
$$\text{(By difference) } As = 37.80$$
$$100.00$$

$$Zn : As = 959 : 504 = 1.902 : 1$$

$$Zn_2As$$

Zinc acts toward arsenic vapors like nickel; the ratio 2 / 1 seems to be the normal.

7. Action of Arsenic Vapors upon Lead.

The lead melts. Exposure for twenty hours. Product is still

malleable to some extent, but breaks off short when nicked with the chisel and then bent. Looks homogeneous.

Analysis gave:

Pb =	96.10
As =	3.75
	99.85

$$Pb : As = 0.4637 : 0.050 = 9.27 : 1$$

The ratio is that of whitneyite. The ionic mobility is small.

MICHIGAN COLLEGE OF MINES, May, 1903.

CRYSTALLOGRAPHIC PROPERTIES.

BY FRED EUGENE WRIGHT.

The following group of artificial minerals, which Dr. Koenig has kindly intrusted to me for crystallographic investigation, is characterized by high metallic luster, tin-white to steel-gray and even black color, conchoidal fracture and hardness: 3–4. The crystals obtained are all small and rarely exceed a millimeter in length. The character and quality of the crystal faces is not uniform for the whole group. Those of domeykite are usually sharp, well formed and furnished single reflection signals on the goniometer, whereby a fairly exact determination of its elements could be obtained. The crystals of the remaining minerals are far less perfect, their uneven undulating faces evincing such indistinct, manifold reflexion signals on the goniometer that an accurate determination of their elements was impossible.

The crystals were measured on a Goldschmidt's two-circled goniometer (model 1901), with attachment for observing small, weak faces and an electric arc goniometer lamp.[1]

[1] The electric goniometer lamp (Fig. A) consists of a box (*a*) made of Russian sheet iron, lined with asbestos cardboard, and of an electric lamp (*b*). The back of the box (*a*) is left open to allow the insertion of the electric lamp—a small black movable curtain serving to shut off all false light, which might disturb in measuring. The mirror (*d*) and cap (*e*) are used to light the verniers and were taken bodily from a Goldschmidt's Auer burner gas goniometer lamp (see *Zeitschrift für Krystallographie*, Bd. xxiii, 1894, p. 149. V. Goldschmidt. Eine neue Goniometer-lampe). The electric lamp (*b*) is the No. 10, hand feed, of J. B. Colt & Co, Chicago, Ill. (price, $10), together with an adjustable rheostat (seven to eighteen ampères, $13). By means of the latter the intensity

DOMEYKITE.

Twelve domeykite crystals were measured, all of which indicated the holohedral division of the hexagonal system with the following observed forms:

Letter:	*c*	*b*	*a*	*s*	*v*	*p*	*x*
Symbol Bravais	0001	$10\bar{1}0$	$11\bar{2}0$	$20\bar{2}3$	$10\bar{1}1$	$20\bar{2}1$	$11\bar{2}2$
Goldschmidt	0	∞ 0	∞	$\frac{2}{3}$ 0	1 0	· 2 0	$\frac{1}{2}$

Also two uncertain forms? $\frac{1}{6}$ 0 ($10\bar{1}6$), which occurred only once

---

of the light may be regulated. An ammeter may be used, but is hardly necessary. In the slits in the tube (*f*) two small colored (blue and green) pieces of glass are placed, which cut down the intensity of the light and impart to it a color restful to the eye. In case very bright light is needed one of the glass plates

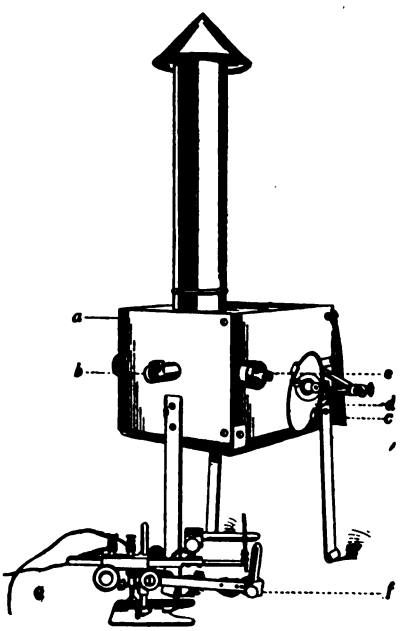


Fig. A.

may be removed. The carbons are observed through a round piece of glass fitted in the cap (*e*). The electric arc goniometer lamp is especially adapted for the measurement of etch figures and minute crystal faces, but serves equally well for ordinary measurements as the light intensity may be regulated and cut down at will.

as a sharp, perhaps vicinal face on crystal No. 4; and ? $\frac{1}{2}$ 1 ($12\bar{3}2$), observed once as a small rounded face (crystal No. 3).

Element: $p_0 = 1.0259 \pm 0.0006$
$a : c_{10} = 1 : 0.8885$ [1]
$a : c_1 = 1 : 1.539$

The element p_0 was calculated from the mean of the angles of 27 sharp, single reflecting pyramidal faces of the measured crystals, no attempt being made to separate the different types of crystal habit. The form *v*, 10 furnished 12 of these faces ($\varphi = 30° 00'$, $\rho = 45° 44'$, possible error $\pm 1'$), the form *p*, 20, 11 faces ($\varphi = 30° 00'$, $\rho = 64° 01'$, possible error $\pm 3'$), and the form *z*, $\frac{2}{3}$ 0, 4 faces ($= 30° 00'$, $\rho = 34° 23'$, possible error $\pm 1'$).

Table of Angles.[2]

c—1′539	lg c—018721	lg a_0—005135	lg p_0—001112	a_0—1′1255	p_0—1′0259	(G_1)

No.	Letter	Symbol	Bravais	ϕ	ρ	ξ_0	η_0	ξ	η	x (*Prisms*) ($x : y$)	y	d — lg ρ
				°	°	°	°	°	°			
1	*c*	0	0001	—	0.00	0.00	0.00	0.00	0.00	0	0	0
2	? *a*	∞	$11\bar{2}0$	30.00	90.00	90.00	90.00	30.00	60.00	0′.5 773	∞	∞
3	*b*	∞ 0	$10\bar{1}0$	0.00	90.00	0.00	90.00	0.00	90.00	0	∞	∞
4	*z*	$\frac{2}{3}$ 0	$20\bar{2}3$	0.00	34.23	0.00	34.23	0.00	34.23	0	0′.6840	0′.6840
5	*v*	1 0	$10\bar{1}1$	0.00	45.44	0.00	45.44	0.00	45.44	0	1′.0259	1′.0259
6	*p*	2 0	$20\bar{2}1$	0.00	64.01	0.00	64.01	0.00	64.01	0	2′.0518	2′.0518
7	? *x*	$\frac{1}{2}$	$11\bar{2}2$	30.00	41.37	23.57	37.34	19.23	35.06	0′.4443	0′.7730	0′.8885

Description of the Individual Forms.

1. The form *c*, 0 (0001) was absent on only one crystal (No. 9, Fig. 4). It has sharp hexagonal outline, splendent metallic luster and is usually perfectly flat, except in the larger crystals where it is frequently uneven and undulating. If observed under a micro-

[1] $a : c_{10}$ denotes the axial relation of the pyramid 10 ($10\bar{1}1$) and $a : c_1$ those of the pyramid 1 ($11\bar{2}1$). See V. Goldschmidt, *Index der Krystalformen*, Vol. i, p. 35.

[2] Calculated according to scheme given in *Winkeltabellen*, von V. Goldschmidt.

scope lighted by auto-colimation the basal plane frequently appears covered with three systems of fine lines, cutting one another at an angle of 60° and running parallel to the outer edges of the crystal. The striæ are usually so fine that they are invisible to the naked eye and have no effect on the sharp reflexion signal from the face.

2. The form a, ∞ $(11\bar{2}0)$ is rare, occurring but once on one crystal, and then somewhat rounded. From the correct position of the reflexion signal, however, the character of the zone and relations

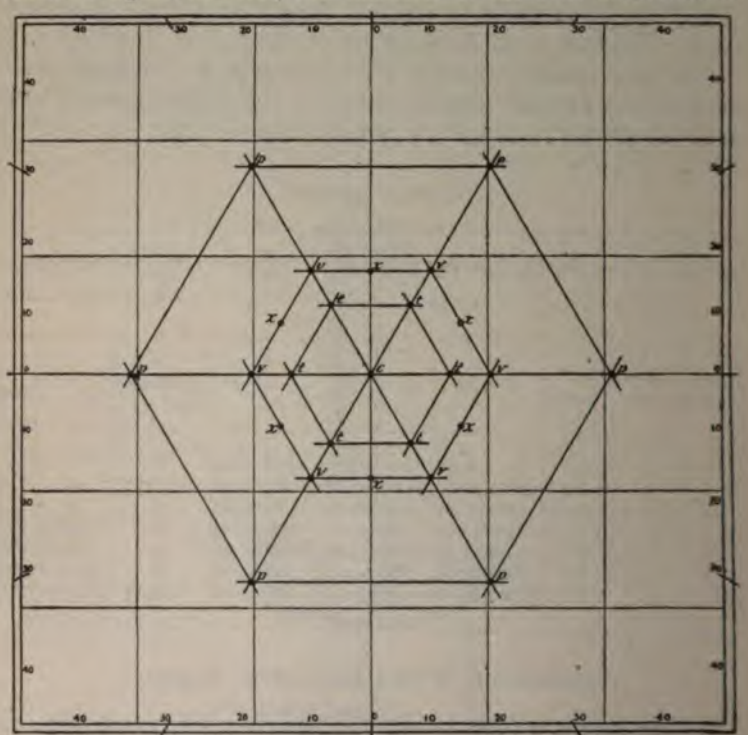


Fig. 1.

to the other faces, the form was considered probable. Its position on the crystal is given in Fig. 4.

3. The form b $(10\bar{1}0)$, ∞ o was observed on ten of the twelve measured crystals, often perfectly even and flat, at times, however, striated parallel to the basal edge—a phenomenon especially noticeable on the hollow skeleton-like crystals (Fig. 3), and due probably to the imperfect formation of the face.

The character of the three pyramidal faces *s*, *v*, *p*, is uniform and unvaried. Long narrow faces exhiting occasionally fine striæ parallel to the basal edge, particularly on the hollow crystals. The form *s*, $\frac{2}{3}$ o (20$\bar{2}$3) appeared on three of the measured crys-

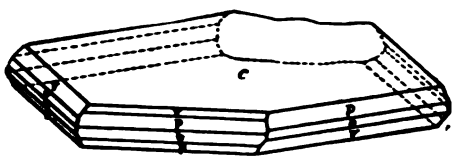


Fig. 2.

tals, *v*, 10 (10$\bar{1}$1) on ten, and the form *p*, 20 (20$\bar{2}$1) on ten and on one crystal as the only pyramid (Fig. 5).

The form *x*, $\frac{1}{2}$ (11$\bar{2}$2), like *a*, ∞ o, occurred but once and on the same crystal with the latter, as a small rounded face (Fig. 4).

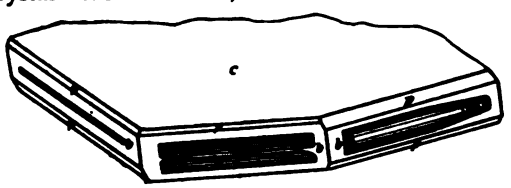


Fig. 3.

Its reflexion signal was approximately in its correct position. The form is considered probable.

Fig. 1 gives a gnomonic projection of all the observed forms on domeykite.

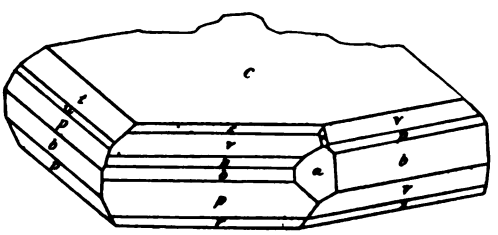


Fig. 4.

Types of Crystal Habit.—Fig. 2 (crystal No 1, dimensions 1.2 × 1.2 × 0.3 mm.) illustrates the first and most common type of domeykite crystals. Flat, thin tabular cystals with the forms o, ∞ o, 10, 20, the last three as narrow, sharp faces frequently striated horizontally. These artificial crystals are occa-

sionally hollow and resemble then Fig. 3 (crystal No. 8, 1.1 × 1.0 × 0.4 mm.). The central part of the prism faces is not filled out as yet, while the basal plane is perfectly flat. although in some cases a mere fragile paper-like film. The pyramid faces are usually present in the form of narrow strips. The hollow parts of the crystals seem to consist of a pile of innumerable thin plates piled one on top of the other, similar to the structure of a biotite crystal. The first type passes gradually into a thicker tabular one, in which the pyramid faces are more prominent. Fig. 4 (crystal No. 5, 0.9 × 0.9 × 0.5 mm.). In one case the steep pyramid *p*, 20,



Fig. 5.

with *b*, ∞ 0, were the only faces developed. Fig. 5 (crystal No. 9, 1 × 0.5 × 0.6 mm.). Fine horizontal striæ were then apparent on all faces of the crystal. Rounded transitional faces between *v* and *c* were noticed on one crystal.

Of the measured crystals No. 1 (with the observed forms *c*, *v*, *p*, *b*), No. 7 (with *c*, *v*, *p*, *b*), No. 10 (with *c*, *v*, *p*, *b*), No. 12 (with *c*, *v*,

p, *b*) belonged to the first type of crystal habit; No. 8 (with *c*, *p*, *b*) to the second; No. 2 (with *c*, *z*, *v*, *b*), No. 3 (with *c*, *v*, *p*, *b*), No. 4 (*c*, *v*, *b*, $\frac{1}{6}$, o (?)) No. 5 (*c*, *z*, *v*, *p*, *a*, *b*, *x*), No. 6 (*c*, *z*, *v*, *p*, *b*) and No. 11 (*c*, *v*, *p*) to the third, and No. 9 (with *p*, *b*) to the fourth.

Development of the Crystal Forms.—The only zone of any consequence present is that of the pyramid from the base to the prism. Considering the base and prism as primary forces the series reads *c*, *z*, *v*, *p*, *b* or o $\frac{2}{3}$ 1 2 ∞. This series is normal except for the second member $\frac{2}{3}$. The sense of this variation is not apparent. The primary forces have produced in the domeykite crystals the most common crystal faces. The simple dominant of the series *v*, 10, and the form *p*, 20, appear equally well developed, while the most highly differentiated form *z*, $\frac{2}{3}$ o occurs least frequently.

The crystals of domeykite tarnish easily to iridescence, and are then unfit for measurement. Luster of fresh crystals splendent metallic; color tin-white to steel-gray in reflected light, black in diffused light, often with a tinge of red. Fracture conchoidal, uneven. Brittle. Indications of a cleavage or parting after the base may result from the leafy texture mentioned above. Cleavage after the prism *a* (11$\bar{2}$0)) ∞ imperfect but distinctly noticeable. The specific gravity was left undetermined, as sufficient material for an accurate measurement was not available.

Etch Figures.

In these experiments the etch figures on the basal plane only were observed. This face is the largest and most perfect, and the one best adapted to reveal the crystallographic nature of the mineral. The other faces, moreover, occur only as narrow bands, frequently striated horizontally—two features detrimental to the formation of good etch figures.

In the process of etching, the minute domeykite crystals were placed in a small receptacle or holder made of finely woven platinum wire meshes, attached to which was a long handle of thicker wire, and then dipped into the acid, allowed to remain there a certain length of time, finally removed by means of the thick wire handle and plunged quickly into water. In this manner it was pos-

[1] Compare V. Goldschmidt, "Ueber Entwickelung der Krystallformen," *Zeitschrift für Krystallographie*, Bd. xxviii, p. 1–35, 419–451.

sible to measure the time of attack or exposition of the acid exactly to a second.

Etching acids used were NO_3H and HCl. The action of these two acids on the domeykite crystals is totally different. The nitric acid works energetically and causes a strong development of gas which keeps the submerged crystal in constant motion. The hydrochloric acid in contrast attacks the crystals very slowly (even when heated), causes no gas bubbles but becomes gradually colored yellow.

Nitric Acid.—With this acid the best results were obtained with a cold (15° C.) dilute solution of four parts concentrated nitric acid (70.6 %, sp. gr. = 1.426) and five parts water, with time of exposition 10″–20″. On one crystal unusually sharp etch figures were observed after an exposition of 13″. A long series of different concentrations were tried (from concentrated NO_3H down to 1 : 2 and lower). The times exposed varied from 10″ up to 120″. Nitric acid appears to etch most rapidly parallel to the outer edges of the crystal. The etch figures are very small, remarkably flat and shallow; exhibit generally sharp hexagonal outline, and grade especially on too long exposition or too concentrated acid into a hexagonal network of three systems of straight lines running parallel to the outer edges of the crystal. Compare the following figures:

Fig. 9	(dilution	4 : 5.	Time of	exposition	30″.	Magnification	40 ×).
Fig. 10	"	4 : 5.	"	"	20″.	"	60 ×).
Fig. 11	"	4 : 5.	"	"	40″.	"	40 ×).
Fig. 12	"	3 : 4.	"	"	60″.	"	60 ×).

On one crystal, however, three-sided figures were observed, their slightly convex lines running parallel to the outer edges of the crystal. On several crystals one of the three systems of lines appeared in certain parts of the field to be less strongly developed than the remaining two sets, while in other parts of the field a second system was absent, etc. (Fig. 11). The rule seems to hold good in such cases that in the near vicinity of an outer edge that system of lines is poorly developed which runs parallel to the edge. The outer edge seems to have had a certain influence on the development of the lines of the etch figures. Usually, however, all three sets of lines are equally well formed (Fig. 12). The etch figures are so small that they give no noticeable reflexion signals on the goniometer.

Hydrochloric Acid.—Hydrochloric acid attacks domeykite under



FIG. 9.



FIG. 10.



FIG. 12.



FIG. 11.



all conditions of concentration and temperature only very slightly. In one experiment with cold HCl (1 part concentrated HCl 30.5 %, sp. g. = 1.515 with 1 part water, time of exposition 7′), however, very small etch figures with sharp hexagonal outline were produced, not unlike those resulting from nitric acid. Their edges ran also parallel to the outer crystal edges. The absence of one set of parallel lines in the vicinity of that outer edge to which it was parallel was also observed on one crystal, etched with HCl. It is noteworthy that in this chemical process no noticeable gas bubbles are seen to escape.

Both the crystallographic measurements and the etch figures seem thus to prove the hexagonal nature of artificial domeykite crystals. On the following minerals, however: argentodomeykite, stibiodomeykite and mohawkite, the basal plane was so poorly developed that good, trustworthy etch figures could not be obtained. Their crystallographic system was deduced solely from the goniometric measurements.

In a recent article[1] on artificial domeykite crystals, Mr. Stevanovics considers the crystals examined by him to be orthorhombic, notwithstanding the hexagonal symmetry of his measurements, and bases his conclusions on the appearance of a cleavage after the macropinacoid, 100. A careful investigation by the present writer confirmed the cleavage noted above after three faces 60° apart. The cleavage seemed equally good after all three faces. In certain pieces cleavage fragments of perfect hexagonal outline (equilateral triangles) were produced. On the goniometer the angle between two such cleavage faces was found to be approximately 60°. The basal plane was uneven and did not permit an exact adjustment of the crystal.

The elements and forms described by Mr. Stevanovics were the following:

Orthorhombic: $a : b : c = 0.5771 : 1 : 1.026$

Forms:	*c*	*m*	*b*	*p*	*d*	*v*	*g*	*z*	*e*	*r*?	*t*?	*q*?
	001	110	010	111	021	112	011	113	023	043	041	0.5.12

with *t*, *r*, *q* rare and uncertain.

As hexagonal crystals these elements and forms become:

[1] *Zeitschr. f. Krystallographie*, Vol. xxxvii, pp. 245–246, 1903.

Element: $p_0 = 1.0206$

$a : c_{10} = 1 : 0.8838$

$a : c_1 = 1 : 1.531$

Forms:	c	m, b	t (?)	d, p	g, v	r (?)	e, z	q (?)
Bravais.....	.0001	10$\bar{1}$0	40$\bar{4}$1	20$\bar{2}$1	10$\bar{1}$1	40$\bar{4}$3	20$\bar{2}$3	50$\bar{5}$12
Gdt.....	0	∞ 0	40	20	10	$\frac{4}{3}$	$\frac{2}{3}$ 0	$\frac{5}{12}$ 0

ARGENTODOMEYKITE.

The crystals of the artificial argentodomeykite belong also to the holohedral division of the hexagonal system. Four crystals only were measured, each crystal exhibiting slightly different elements, due probably to a varying percentage of silver. For the form p, 20 of the several crystals

The angle $\rho = 65° 14' \pm 29'$, $p_0 = 2.167 \pm 0.047$, in the first crystal.
$\rho = 65° 08' \pm 20'$, $p_0 = 2.158 \pm 0.032$, in the second.
$\rho = 64° 33' \pm 11'$, $p_0 = 2.101 \pm 0.017$, in the third.
$\rho = 64° 36' \pm 15'$, $p_0 = 2.106 \pm 0.024$, in the fourth.

Although the above quantities vary considerably, still they show clearly that the entrance of the silver in the domeykite crystal particle causes a change in its elements, the pyramids becoming steeper.



Fig. 6.

The quality of the faces of these artificial crystals was so poor that the influence of the silver in per cent. could not be determined.

Size of crystals and character of faces similar to domeykite. Of the pyramids the form p, 20 predominates, the form v, 10 occurred but once, while p, $\frac{3}{2}$ 0 was not observed. The face p, 20 frequently exhibits a slight cylindrical rounding, the axis of which runs parallel to the basal edge. Its reflexion signal is then a short light band, its bright central part indicating the position of the face. The faces are invariably striated horizontally. The different types of

crystal habit are illustrated in Figs. 6 and 7 (dimensions of crystals 0.9 × 0.9 × 0.4 mm. and 1 x 0.5 × 0.4 mm.).

The external appearance of the artificial argentodomeykite resembles that of domeykite. Its color is perhaps more nearly silver-white. The crystals tarnish easily and become iridescent.

Stibiodomeykite.

The artificial products of this mineral were not suitable for goniometric measurement. The faces were without exception uneven. An examination of the various preparations with a pocket lens revealed two different types of crystal habit—the first of them tabular with the base predominating, the other faces practically undeveloped; the second long prismatic, almost arrow-shaped, the horizontally striated pyramid faces terminating either in a short point

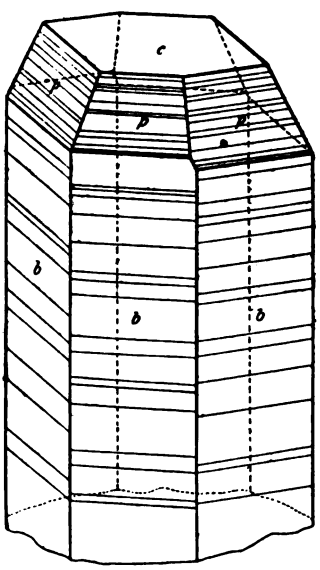


Fig. 7.

or becoming wider at the top, and resembling then an overturned bottle or inverted cone.

Color, light steel-gray. Tarnishes less readily than preceding minerals and iridescence rarely noticeable. Fracture conchoidal. H = 3–4.

person qualified for the task shall take up this highly important subject, the languages and the customs of an interesting primitive people will be lost to science.

The languages spoken by the native inhabitants of the New England district of New South Wales are quite different in vocabulary and intonation from those found in any other part of New South Wales which I have visited. Therefore I consider myself very fortunate in being the first author to report their grammatical structure.

In the following pages I shall endeavor to record and preserve the elements of two aboriginal languages, with a vocabulary of one of them. All of the materials of the grammars, and also of the vocabulary, have been collected by me in the camps of the aborigines, and were noted down direct from the mouths of the native speakers, so that I can become entirely responsible for their accuracy.

In common with other Australian languages reported by me, the Anēwan and Banbai tongues possess a double form of the first person of the dual and plural, in every part of speech subject to inflection, by means of which the person spoken to may be included or excluded. It may be stated here that I was the first author to give full details of this peculiarity in the languages of Australia,[1] although it had been observed to a certain extent in some of the islands of the Pacific Ocean, and among the Amarinds of North America. These two languages likewise contain a dual and plural number in all parts of speech.

It is hoped that these efforts of mine may prove of some value, by enabling philologists to compare the native tongues of Australian tribes, not only among themselves, but with other languages in the islands of Polynesia, Melanesia, and various parts of the Pacific Ocean, as well as with the speech of other primitive tribes in different parts of the world.

The space at my disposal in the PROCEEDINGS of this Society render it necessary to describe only the leading elements of the languages dealt with.

ORTHOGRAPHY.

The system of orthoepy adopted is that which is recommended by the Royal Geographical Society of England, but a few addi-

[1] "The Gundungurra Language," PROC. AMER. PHIL. SOC., Vol. xl, p. 140.

tional forms of spelling have been incorporated, to meet the requirements of the Australian pronunciation, as follows:

As far as possible, vowels are unmarked, but in some instances the long sound of *a*, *e* and *u* are indicated thus, ā, ē, ū. In a few cases the short sound of *u* has been marked thus, ŭ.

G is hard in all cases. *R* has a rough, trilled sound, as in the English word "hurrah!" *W* always commences a syllable or word.

Ng at the beginning of a word or syllable has a peculiar nasal sound. At the end of a syllable or word it has substantially the sound of *ng* in the English word "sing."

The sound of the Spanish ñ is frequent; at the beginning of a word or syllable I have given it as *ny*, but when terminating a word the Spanish ñ is used. *Y* at the beginning of a word has its ordinary consonant value.

Dh is pronounced nearly as *th* in the English word "that," with a slight sound of *d* preceding it. *Nh* has also nearly the sound of *th* in "that," but with a slight initial sound of the *n*.

T is interchangeable with *d*; *p* with *b*; and *g* with *k*.

Ty and *dy* at the commencement of a word or syllable have nearly the sound of the English *j*, or the Spanish *ch*; thus, *dya* or *tya* closely resemble *ja* or *cha*. At the end of a word or syllable *ty* is sounded as one letter, closely approaching the *tch* in the English word "catch," but omitting the final hissing sound.

In all cases where there is a double consonant, each letter is enunciated.

The Anewan Language.

The remnants of the Anēwan tribe are scattered over the southern half of what is known as the "table-land" of New England, including Macdonald river, Walcha, Uralla, Bendemeer, Armidale, Hillgrove and other places.

ARTICLES.

The indefinite article, *a*, is not represented, but the demonstrative pronouns, in their numerous modifications, supply the place of the definite article, as "this man," "that woman," "yonder hill." The English adverb, *here*, in its several native forms, is frequently treated as a demonstrative, and is then also a substitute for the definite article.

NOUNS.

Nouns have number, gender and case.

Number.—There are three numbers—singular, dual and plural. Kana, a crow. Kanaburala, a pair of crows. Kananyeta, several or many crows.

Gender.—Gender in the human family is denoted by different words. Tana, a man. Kettyura, a woman. Romunna, a boy. Kĕmika or nganda, a girl. Kwanga, a child of either sex.

Among animals gender is distinguished by using words signifying "male" and "female." Pwēla, an opossum. Pwēla rula, a male opossum. Pwēla imbarra, female opossum.

Case.—The principal cases are the nominative, causative, instrumental, possessive, accusative, dative and ablative.

Nominative: This case simply names the subject, as imboanda, a kangaroo; naia, a yamstick, without any change in the noun.

Causative: When a transitive verb is used the noun takes a suffix, as Tananda imboanda nyuna, a man a kangaroo is beating. Kettyuranda pwēla nyuna, a woman an opossum is beating.

Instrumental: This takes the same suffix as the causative. Kettyuranda tana nyuna naianda, a woman a man is beating with a yamstick. Tananda imboanda nyūmbina arkananda, a man a kangaroo hit with a boomerang.

Possessive: Tanango arkana, a man's boomerang. Kettyurango naia, a woman's yamstick.

Accusative: This is the same as the nominative.

Dative: Rullagu, to a camp.

Ablative: Rullūnge, from a camp.

It should be mentioned that in all the expressions illustrating the several cases, both in the Anēwan and Banbai languages, the demonstrative pronouns are omitted, for the two-fold purpose of saving space and of avoiding confusion by introducing any more words than are really necessary to show the declension. For example, where I have given "man kangaroo hit with boomerang" would be fully expressed by the native thus: "Man this-on-my-right kangaroo yonder-in-front boomerang struck-with," or as the subject might require.

These remarks apply to every example of aboriginal sentences throughout both the languages dealt with in this article.

ADJECTIVES.

Adjectives succeed the nouns they qualify, and take the same inflections for number and case.

Tana birkungirra, a man large.

Tanango birkungirrango arkana, a large man's boomerang.

Tananda birkungirranda kwanga nyuna, a large man is beating a child.

It is not necessary to give examples of the other cases.

Comparison of adjectives is effected by two positive statements, such as, This is good—that is bad: runyerra indya—irrunga indyunda.

PRONOUNS.

Pronouns have three numbers, with inclusive and exclusive forms in the first person of the dual and plural. The following table exhibits the nominative pronouns:

Singular	1st Person	I	Yukka
	2d "	Thou	Indyukka
	3d "	He	Gambaua
Dual	1st Person	We, incl.,	Tēka
		We, excl.,	Tāla
	2d "	You	Twukka
	3d "	They	Takana
Plural	1st Person	We, incl.,	Nyukka
		We, excl.,	Nāla
	2d "	You	Audilla
	3d "	They	Nalena

The possessive and objective pronouns are as under:

Singular.

1st Person	Mine	Yinga	Me	Enna
2d "	Thine	Nyunga	Thee	Nunya
3d "	His	Onning	Him	Onna
1st Person	Ours, inc.,	Tenyunga	Us, incl.,	Tenya
	Ours, excl.,	Tambiga	Us, excl.,	Tuanya
2d "	Yours	Twanyung	You	
3d "	Theirs	Lambiga	Them	Walanya
1st Person	Ours, incl..	Nyambiga	Us, incl.,	Nanyabura
	Ours, excl.,	Nyanyambiga	Us, excl.,	Nanyumbinga
2d "	Yours	Nuka	You	Audumbinga
3d "	Theirs	Nambiga	Them	Nanya

There are forms of the pronouns meaning "away from me," "towards me," etc., which must be passed over for want of space.

Interrogatives: Who, ānunga. What, nyanga. What for, nyangabura.

Demonstratives: This, indya. That, indyunda. The demonstratives are numerous, and of various forms, frequently taking the place of pronouns of the third person in the singular, dual and plural. This accounts for the great diversity of the third personal pronouns, which have little or no etymological connection with the others.

The demonstratives in this language, by the combination of simple root-words, can be made to indicate position, direction, distance, movement, possession, number, person and size. If space permitted, I could show tables of these demonstratives which would be most important for comparative purposes. This applies also to the Banbai demonstratives.

VERBS.

Verbs have the singular, dual and plural numbers, with the usual tenses and moods. There is a form of the verb for each tense, which remains constant through all the persons and numbers of that tense. Any person and number can be expressed by using the required pronoun from the table given in the foregoing page.

Following is a short conjugation of the verb Nyuka, "to beat or strike."

Indicative Mood—Present Tense.

Singular	1st Person.....I beat		Yukka nyuna
	2d " .....Thou beatest		Indyukka nyuna
	3d " .....He beats		Gambaua nyuna

and so on through the dual and plural.

Past Tense.

Singular.....	1st Person.....I beat	Yukka nyumbina

Future Tense.

Singular.....	1st Person.....I will beat	Yukka nyumarala

Imperative Mood.

Beat, nyumera	Beat not, yinna nyumera

Conditional Mood.

Perhaps I will beat	Yukka neta nyumarala

Reflexive.

Present..I beat myself	Yukka nyugatina
Past ....I beat myself	Yukka nyugatimbina
Future ..I will beat myself	Yukka nyugatila

Reciprocal.

Dual....We, exclusive, are beating each other,	Tāla nyutaka
Plural...We, exclusive, are beating each other,	Nāla nyutaka

ADVERBS.

The following are a few of the more commonly used adverbs:

Yes, ngeh. No, apala. Today, lunna. Tomorrow, yūn. Soon, lanabura. By and bye, loka. Long ago, toangga. Now, ilan. Recently, irrandya.

How, thanggana. Where, renya. How many, thambula. Here, āwa. There, gamba. The two last are frequently used as demonstratives.

PREPOSITIONS.

In the rear, yanda. In front, gattanda. Around, lunggai. In the middle, umūnda. Up, dapai. Down, irrakirran. Between, ilkongga.

CONJUNCTIONS.

The general absence of conjunctions is attributable to the numerous modifications of the different parts of speech, by means of which sentences are brought together without the help of connecting words.

INTERJECTIONS AND EXCLAMATIONS.

These parts of speech are not numerous.

NUMERALS.

One, nyoanda. Two, tuala.

The Banbai Language.

The aboriginal tribes speaking this language adjoin the Anéwan community on the north, and are located at Guyra, Ben-Lomond, Wollomombi and Kookarabooka.

NOUNS.

Number.—There is no special declension for number, but the noun is followed by words signifying two or several.

Ginggēr bulabulari, kangaroos two.

Ginggēr girrawa, kangaroos several.

Gender.—Man, thaimburra. Woman, burranyen. Boy, bodyerra. Girl, dillanggan. The sex of animals is denoted by words meaning "male" and "female" respectively, placed after the creature's name, as, Margan dyillawara, a buck wallaby. Margan kandura, a doe wallaby. Among birds, boro means a cock, and ngapara, a hen.

Case.—There are the nominative, causative, instrumental, possessive, accusative, dative and ablative cases.

Nominative: Tua, a boomerang. Kunnai, a yamstick. Wandyi, a dog.

Causative: Ginggēru nganya bittang, a kangaroo me scratched. Burranyendu nganya buang, a woman me struck.

Instrumental: Thaimburradu nganya bindaimang tuandu, a man at me threw a boomerang.

Possessive: Burranyengu kunnai, a woman's yamstick. Thaimburrangu tua, a man's boomerang.

In the Gundungurra, and in several other aboriginal languages of New South Wales and Victoria, the article possessed takes a suffix, as well as the possessor. For example, warrangan means a boomerang, and murriñ a man, but "a man's boomerang" must be expressed, Murrin-*gu* warrangan-*gung*. Until reported by me,[1] this peculiarity of a double suffix in the genitive case of Australian nouns had not been observed by any previous author.

Dative: Nguralami, to a camp.

Ablative: Nguranga, from a camp.

Accusative: This is the same as the nominative.

ADJECTIVES.

Adjectives take the same inflections as the nouns which they qualify.

Thaimburra burwai, a man large.

Thaimburradu burwaidu nganya buang, a man large me struck.

Thaimburrangu burwaigu tua, a large man's boomerang.

[1] "The Gundungurra Language," PROC. AMER. PHIL. SOC., Vol. xl, p. 143.

Comparison: Nyam dhurrui—nyam yōnggo; this is good—that is bad. Nyam dhurruiūnba, this is very good.

PRONOUNS.

Pronouns have the nominative, possessive and objective cases, as in the subjoined tables. There are two forms in the first person of the dual and plural—one in which the person or persons addressed are included with the speaker, and another in which they are exclusive of the speaker. The following is a list of the pronouns in the nominative case:

Singular	1st Person.....I		Ngaia
	2d " .....Thou		Nginda
	3d " .....He		Ngurrung
Dual	1st Person...	We, incl.,	Ngulli
		We, excl.,	Ngulligai
	2d " ...	You	Bulala
	3d " ...	They	Bulagai
Plural	1st Person...	We, incl.,	Nyeūlla
		We, excl.,	Nyeūllagai
	2d " ...	You	Nguddyilindya
	3d " ...	They	Yangbēndu

The possessive and objective forms of the pronouns are exhibited in the following table:

Singular.

1st Person.....Mine		Ngunyo	Me	Nganya
2d " .....Thine		Nginnu	Thee	Ngēna
3d " .....His		Gurragunga	Him	Nyam

Dual.

1st Person ..	Ours, incl.,	Ngullimba	Us, incl.,	Ngullinya
	Ours, excl.,	Ngullimbagai	Us, excl.,	Ngullinyagai
2d " ...	Yours	Bullamba	You	Bulanya
3d " ...	Theirs	Bullambagai	Them	Bulanyagai

Plural.

1st Person ..	Ours, incl.,	Ngeūmba	Us, incl.,	Ngeanya
	Ours, excl.,	Ngeumbagai	Us, excl.,	Ngeanyagai
2d " ...	Yours	Nguddyimba	You	Nguddyinninya
3d " ...	Theirs	Ittyāran	Them	Ittyārambēn

There are also forms meaning "with me," nganyumbulla. Ngaia, as in the table, is used with an intransitive verb, as, ngaia nganggi, I sit; but when a transitive verb is used, the pronoun is changed to ngatya, as, Ngatya bōnggi, I beat. These rules apply to the other persons and numbers. Other forms of the pronouns are omitted for want of space.

Interrogative pronouns: Who, wuttanya. Whom belonging to, wuttanyannin. What, minya. How many, minya-minya.

Demonstrative pronouns: This, nyam. That, mumum. These are frequently used as adverbs, and they mean "here" and "there."

VERBS.

The rules for the conjugation of verbs are similar to those of the Anēwan language. An example in the singular number of each tense will be sufficient:

Indicative Mood—Present Tense.

Singular	1st Person.....I beat		Ngatya bōnggi
	2d " .....Thou beatest		Nginda bōnggi
	3d " .....He beats		Ngurrung bōnggi

Past Tense.

Singular.....	1st Person.....I beat		Ngatya boang

Future Tense.

Singular.....	1st Person.....I will beat		Ngatya bōanggo

The imperative, conditional, reflexive and reciprocal forms of the verb will be passed over for want of space.

ADVERBS.

Yes, nge. No, wunā. Today or now, gillu. Tomorrow, gurlau. Soon, gurubilli. By and bye, kangā. Long ago, dhullūmba. Yesterday, nyukkumba. Certainly, yare. How, dyirrung. Perhaps, dyirraugam. Where, dyota. How many, minya-minya. Here, nyam. There, mundyaba. Yonder, mungga-munggara. Marēda, far away. Close to speaker, tulbaia.

The adverbs "here" and "there" are often used as demonstrative pronouns, and have the same meaning as "this" and "that."

PREPOSITIONS.

In front, munggara. In rear, wallungga. Between, pimita. On the other side, kawatadyula. On this side, ilāmgidda. Up, kaba. Down, warri. Around, kokari.

Conjunctions and interjections are omitted.

NUMERALS.

One, kurrukun. Two, bulari.

A Mystic or Secret Language.

Before concluding this short article on the speech of the Australian aborigines, I wish to refer to a secret language, used by the men at the ceremonies of initiation, but which is never spoken in the presence of women, or in the presence of those youths who have not yet entered upon the prescribed course of initiation. Whilst the novitiates are away in the bush in charge of the elders of the tribe, they are taught a mystic name for surrounding objects of every-day life, for animals, for parts of the human body, and short sentences of general utility. This language is different in different tribes.

I was the first author to draw attention to this mystic tongue,[1] and during the past year I contributed to the Royal Society of New South Wales some vocabularies of the secret languages of the Kurnu[2] and other Australian tribes. I consider my discovery of this secret form of speech is of great linguistic importance, and invite my readers to peruse the vocabularies referred to.

In connection with this subject it may be mentioned that in 1901 I contributed an article to the Royal Geographical Society of Queensland, on some "Aboriginal Songs at Initiation Ceremonies,"[3] in which I published several sacred chants in the secret tongue, which are the first songs of the kind ever set to music.

Vocabulary of Anewan Words.

The following vocabulary, containing about 210 of the most important words in general use by the Anēwan tribes, has been prepared by me from notes taken in the camps of the aborigines.

[1] *Journ. Anthrop. Inst.*, London, Vol. xxv, p. 310.

[2] *Journ. Roy. Soc. N. S. Wales*, Vol. xxxvi, pp. 157–160.

[3] *Queensland Geographical Journal*, Vol. xvii, pp. 61–63.

Every word was carefully written down by myself from the mouths of the natives.

In a communication to the Royal Society of Victoria in the year 1896,[1] I gave a comprehensive description of the initiation ceremonies of the Anēwan, Banbai and other tribes. Again in 1897, I contributed a paper to the Royal Society of New South Wales,[2] in which I described the Anēwan laws of marriage and descent, with lists of their *totems*. On account of the two articles referred to, it has not now been thought necessary to repeat the subjects therein dealt with.

English.	*Anēwan.*
Man	tana
Boy	rumunna
Elder brother	irkōmba
Younger brother	ilpaminda
Father	pēta
Woman	kettyura
Girl	kemika
Elder sister	pauana
Younger sister	paua
Mother	irrapella
Child of either sex	kwanga
The Human Body.	
Head	kwulla
Forehead	tui
Hair of head	rella
Beard	nutyina
Eye	ila
Nose	nyanba
Jaw	dhanda
Ear	nakuna
Teeth	yella
Tongue	tŭnda
Navel	dyikanga
Back	twila
Arm	kyŭnda
Shoulder	irringala
Elbow	indina
Hand	nyella
Calf of leg	yula
Thigh	illanba
Knee	gwunba
Foot	nyalla
Heel	nungan
Blood	gwianba
Woman's breasts	ipinda
Fat	pyenna
Skin	twunda
Penis	duna
Testicles	ilwundandha
Semen	bungan
Copulation	bungadala
Masturbation	bungalulamun

[1] "The Bŭrbŭng of the New England Tribes," *Proc. Roy. Soc. Victoria*, Vol ix, N. S., pp. 120–136.

[2] "The Totemic Divisions of Australian Tribes," *Journ. Roy. Soc. N.S. Wales*, Vol. xxxi, pp. 168–170.

English.	*Anēwan.*
Venereal	tharpunda
Anus	būlla
Excrement	ngunba
Urine	itirra

Natural Surroundings.

Sun	nura
Moon	ternda
Stars	ikina
Sky	rūnbinna
Thunder	lāmutika
Lightning	kimmitta
Rain	yūnggara
Fog	ngatta
Snow	ikana
Frost	lala
Hail	arrepanna
Water	ukūnda
The ground	kyuna
Stones	rola
Sand	raikana
Darkness	illona
Coldness	inganna
Fire	inba
Smoke	rutta
Night	lōnna
Food (flesh)	kara
Food (vegetable)	kyaia
Honey	irrōta
Hill	kuta
Watercourse	retta
Any tree	dulla
Leaves of trees	indora
Path	kurra
Shadow	tonba
Summer	ilkaiwa
Winter	tyerwanba

English.	*Anēwan.*
Rainbow	rumira
Large flat rock	lara
Camp	rulla

Mammals.

Kangaroo	imboanda
Porcupine	iwutta
Wild-dog	irritanga
Opossum	pwela
Flying-fox	ramana
Kangaroo-rat	bara
Native-bear	lauanha
Wallaroo	lumulla
Bandicoot	imbunga
Ring-tail opossum	aunda
Native-cat	kyura
Wallaby	kyatta
Tiger-cat	yāra
Bat	lyunganda

Birds.

Birds collectively	pillang
Emu	runda
Eagle-hawk	lambara
Black-duck	rungara
Pelican	wuyara
Laughing jackass	rokala
Crow	kana
Swan	dyuwula
Native-companion	rualgunda
White cockatoo	ērpatha
Black cockatoo	wellara
Common magpie	imbota
Plover	tharringga

English.	*Anĕwan.*
Curlew	rilwinnu
Brown-hawk	ōwara
Parrokeet	imbanga
Mopoke	ūrkūng

Fishes.

Perch	indanga
Jewfish	lyūnda
Codfish	guyu, *or* ruta
Sprat	birran
Eel	indhurra

Reptiles.

Black iguana	rutyala
Water iguana	nhawala
Ground iguana	tyunda
Spotted iguana	laipara
Jew-lizard	nura
Snakes collectively	yenda
Death-adder	minda
Rock-lizard	roppung
Turtle	yiwang
Stinking-turtle	werra
Big frog	imbottonga
Carpet-snake	imbiāla
Sleepy lizard	pwoggana

Invertebrates.

Bee	ronnang
Locust	warra
Centipede	engara
Louse	irrakanba
Nits of lice	minna
House-fly	rulunga
Spider	ālman
Mosquito	irwala

English.	*Anĕwan.*
Bulldog ant (red)	thanda
Bulldog ant (black)	oppunga
Scorpion	imbūnda
Crab	thambanna

Trees and Plants.

Mountain ash	o-inba
Kurrajong	nunggutta
Ironbark	girranba
Stringy bark	indwarra
Wattle	luna
Grass-tree	dunburra
Peppermint	nēwurra
Apple-tree	tūnba
Gum-tree	orrulla
Scrub-gum-tree	bikkara
Pine	wungulla
White box	yina
Reeds	moanda
Forest oak	rēwilla
Cherry-tree	poara
Jeebung	lwainda

Weapons, etc.

War spear	kyenba
Hunting spear	anbelang
Jagged spear	mumberiñ
Spear shield	indūta
Club shield	bekang
Club	raipella
Spear-thrower	womur
Boomerang	arkana
Tomahawk	mukung
Fighting-hook	lēnyang
Nulla-nulla	rularokara
Koolamin	tilla
Net bag	loia

English.	*Anēwan.*
Yamstick	naia
Stone knife	imbōnda
Adjectives.	
Large	birkingirra
Small	latherana
Good	rōnyerra
Bad	irrūnga
Hungry	imbyura
Thirsty	ambia
Quick	ngunna
Slow	numbadia
Afraid	no-aran
Angry	anagana
Greedy	myūna
Verbs.	
Eat	mēka
Drink	imbekka
Sit	nina
Speak	oidekka
Walk	nadiga
Run	nuppanati
Break	wammin
Give	unumbia
Sing	peka
Weep	twaka
Steal	nomekka
Bite	irruttela
Catch	anamarai
Climb	irrukka
Hear	nugguna
Laugh	indeka
Scratch	nirmatin
See	aikunna
Dance	thekinna
Swim	imbwiana
Stand	rāgya
Throw	imbia
Pretend	twandyingan
Swallow	pwika

ON SOME NAMES (CHIEFLY LINNEAN) OF ANIMALS AND PLANTS ERRONEOUSLY PAIRED IN SYNONYMY.

BY MARCHESE ANTONIO DI GREGORIO.

(*Received April 15, 1903.*)

It is well known that a great many new genera have been made for the old Linnean species. One of the chief creators of generic names was Lamarck, the great naturalist. After him a large number of authors have proposed many new genera for the Linnean species. The same is true, also, for many species proposed by ancient authors that have been related in synonymy, when a new genus has been created for the same species.

In my note, "Intornorno ad alcuni nomi di conchiglie linneane," published in the *Bulletin* of the Italian Malacological Society (Vol. x, 1884), I have proposed to retain the original Linnean names for

the species, though this may have been chosen to denote the genus. For instance, the name of *Mya vulsella* L. (as a new genus has been created) has been changed in *Vulsella lingulata*. The name of *Ostrea malleus* L. has been changed in *Malleus vulgaris* Lamk. I have proposed in similar cases to retain the original name of the species, which I believe is the more correct. So I have proposed to call these species *Vulsella vulsella* (L.), *Malleus malleus* (L.).

My proposition has been accepted by many malacologists. Indeed now instead of *Plicatula ramosa* Lamk. (=*Sponotilus plicatus* L.), it is better to employ the name *Plicatula plicata* (L.) sp. Instead of *Lima squamosa* Lamk, (=*Ostrea lima* L.) the name of *Lima lima* (L.) sp.; instead of *Hippopus maculatus* Lamk, (=*Ostrea hippopus* (L.)) the name of *Hippopus hippopus* (L.) sp., etc.

I think that this modification might be conveniently adopted also for plants as well as animals. I believe, for instance, it is much better to say *Tymnus tymnus* than *Scomber tymnus* or *Tymnus vulgaris*. For the same reason I believe it to be much more correct to say *Malus malus* instead of *Pyrus malus* or *Malus communis*.

What I have said for the names of Linné, is also applicable to the names of other authors which have been changed, because recent authors have chosen the name of the species as a generic name.

I call the attention of zoologists and botanists to this interesting innovation. I hope that it will be adopted for plants and for all animals, as it has been for the mollusks.

OBITUARY NOTICES OF MEMBERS DECEASED.

JOSEPH MILLER WILSON, A.M., C.E.

(*Read May 1, 1903.*)

To meet Joseph M. Wilson in ordinary intercourse afforded an opportunity to learn of the good, true and beautiful in human nature; to know him better, in relation to the sciences and arts, was a privilege fraught with instruction, aid and a constant progression onward and upward toward greater knowledge of things as they are; to know him spiritually, to recognize the Holy Spirit of Truth within him dominating all else, was to feel the influence of that more profound, ennobling, holier enlightenment, based upon sincerity and Divine purpose, which "shines reflected in the human face."

To adequately appreciate the strength and force of character which lay back of his admirable work, it is important to recall some of the conditions which brought it about, for, from the varied standpoints of heredity, individual endowment, education and opportunity, Mr. Wilson was favored above many, and the excellent use he made of what he possessed appeals to the very best that philosophy can recognize.

Joseph M. Wilson was a worthy member of a long line of men eminent in professional walks of life, notably as experts in the engineering profession, each of whom had made his mark in public service by ability founded upon integrity, and inspired by a progressive spirit which, while ignoring nothing in nature, conserving the best from the past, ever led onward toward the further betterment of men and things.

His earliest American ancestor was Robert Gibbes (1644–1715), who came from his native place, Barbadoes, to the Province of South Carolina; was Chief Justice of that Province (1708) and Governor (1710–1712). His great-great-grandfather was James Wilson, an engineer and architect of Stirling, Scotland, whose son John Wilson (1755–1798) was Lieutenant in the Seventy-first British Foot (Highlanders), and served throughout the American Revolutionary War as an engineer under Major Moncrief, of the Royal

Engineers; was wounded at the siege of Charleston; later on married there a daughter of Dr. Robert Wilson, a prominent physician of that place, and eventually returned to Scotland.

In the next generation, John Wilson (1789, Scotland–1833, America), the grandfather of the subject of this sketch, having completed his education at the University of Edinburgh, returned (1807) to his mother's native land, and entered upon the profession of engineer and surveyor in Charleston. His professional thoroughness and accuracy are matters of record to this day. A map of South Carolina made by him for the State Government is yet considered the standard authority for all but subsequent improvements. He became a naturalized citizen of the United States at an early period, and during the war with Great Britain (1812) served as engineer for the construction of the works of defense of the city of Charleston; held office of State Civil and Military Engineer for South Carolina under Board of Public Works (1818–1822), and in 1826 removed to Philadelphia, from which time Major John Wilson became identified, as Chief Engineer, with that extensive system of improvements for the State of Pennsylvania which during his life materialized in the Philadelphia & Columbia Railroad. Of his son, William Hasell Wilson (1811–1902), who followed directly in his father's footsteps, the important and enduring work performed by him is still fresh in the memory of many yet living. He was closely identified with the onward march and development of the Pennsylvania Railroad Company's system from its very infancy, when with his father's corps in 1826; through the formative period of the organization, when the standard was set for high achievement which has since obtained; through the Civil War period, when, as Chief Engineer of the Pennsylvania Railroad, his Department of Maintenance and Construction was called upon to preserve intact the highway itself for the transportation of armies and munitions of war as well as the general public and freight traffic; through to the end of a professional career, embracing many positions of trust and responsibility, of exceptional usefulness and duration (actual service, seventy-six years). William Hasell Wilson was finally looked upon as the Nestor of his profession, respected, honored, followed, served by many a younger man of his own corps who can to-day rise up and call his name blessed. Such were some of the influences which operated upon the subject of this sketch.

Joseph M. Wilson possessed by heredity those inestimable privileges and advantages which come from things of good repute in professional life and practice. He was keenly sensitive in upholding the high standard and family traditions thus bequeathed unto him, and he did so with a constancy and fidelity unto himself quite above the control of policies and politics, either secular or religious, which he did not approve.

Born at Phœnixville, Pa. (June 20, 1838); passed a portion of his youth upon his father's farm, Chester county, Pa.; attended private schools, and entered the Rensselaer Polytechnic Institute, Troy, N. Y. (September, 1854). After graduation, with degree of C. E. (1858), took a special course of two years in Analytical Chemistry with Prof. F. A. Genth, at Philadelphia, and in March, 1860, entered the service of the Pennsylvania Railroad Company as Assistant Engineer.

His services with that company covered the important period when the introduction of cast and wrought iron, and later on steel, in lieu of the previous wood-construction for bridges and buildings, involved much original research and experiments more or less novel to the profession. Mr. Wilson was among the early investigators to apply such in actual practice, and became a valued source of information and experience in each of the three departments of this development, viz., mathematical investigation involving the theory of strains, the development of designs, and work executed. His technical papers, published then and later on, form part of the history and literature of the profession in the United States. He held various positions in the service of the company in line of promotion, and as Engineer of Bridges and Buildings embraced opportunities involving increased responsibility by the embodiment of new ideas in construction.

He continued in that service until 1886, during which time he constructed, among many works, the original Broad Street Station, Philadelphia, which for purity in design was eminently characteristic of him.

As early as 1873, when the National Congress and citizens at large became interested in the approaching international celebration of the first centennial of the nation, Mr. Wilson at once took an active part, both as citizen and professionally. Later on, after the adoption of the design for the Main Building, which was finally erected in Fairmount Park, Philadelphia, he became associated with

the writer of this sketch in the building thereof. The Machinery Hall, the first of such dimensions and pretensions in the history of the country, was subsequently designed and erected under similar auspices, Mr. Wilson taking the leading part.

The execution of works having international significance and relations in any field of effort is apt to produce a desire, an impetus to greater comprehensive effort, embodying collateral information and more extended professional skill and practice. Mr. Wilson embodied this progressive spirit in a pre-eminent degree, his qualifications for such progress being excellent, his standard of the highest, his spirit confident. His success which followed is associated with that of his brothers, under the firm-name of Wilson Bros. & Co. (organized 1876), of which he was for many years the senior member. The scope of his work in this relation embraced a field exceptionally large, under auspices much varied, and in locations far distant in other countries; the variety of information necessary to attainment not confined to technical matters, but involving special studies of diverse character, not a few of which resulted in published papers giving much specific data for reference as well as professional opinion. Some of these papers were prepared to aid philanthropy in general, and not only those engaged in the actual construction of buildings. The scope of his work thus became in time very extensive.

Whether in the designing and erection of hospitals or of banking buildings, of comprehensive systems of shops for railroad corporations or structures for industrial enterprises; whether buildings for administrative purposes or Union Depots; whether as engineer or architect, in consultation or for expert testimony, in reference to elevated railways in cities, the water-supply of cities, or the mammoth suspension bridges connecting adjacent centres of population; whether as engineer of subways under municipal control, or as trustee for carrying out of bequests to institutes for the education of future generations; whether as President of the Franklin Institute for ten years, or as member of learned societies, both at home or abroad; whether as author of technical papers for the British Institution of Civil Engineers, London, or as an American author bringing home from France and England his study of trade-schools to improve their construction and administration here—in all these Joseph M. Wilson took part.

His constant desire was for a more comprehensive and advanced

knowledge of actual facts and powers in nature, ever with a view to their direct application in new phases of work, and this kept him in touch with investigation in many fields.

He was never visionary; imagination was not his strong point, not even in his moments of relaxation, when music (the organ in particular), painting or photography were cultivated because of the fine-art appreciation which they called for. Yet few recognized more than he what scientific research contains, potentially, for further advancement by co-operation toward the revelation of truth in unity.

From his point of view this was the highest ideal as to material things, *per se*, demanding constant touch with the progress of the age, and treatment both subjective and objective in life-work.

To study, control and utilize the forces of nature was his business in life, and he did so according to the most approved methods. If this were all, this tribute to his memory might well close at this point with one phrase: a man of high cultivation and refined feeling, an eminent engineer, whose works do follow him. With him, however, this was not all. It was not all of life to live and work—not by any means, neither in science nor religion.

To those who knew him best his personality was most sympathetic, responsive and pure in communication. He was never idle, but constantly seeking in the domain of fine art and kindred fields to gratify a refined taste and keen appreciation of the beautiful as well as the good and true, thus producing impressions which appealed through their spiritual import. These traits characterized his moral nature as forcibly as the more exact sciences and arts appealed to his intellect. His numerous descriptive manuscripts of travel, containing sketches and illustrations drawn on the spot from nature, are as a mine of wealth to those left behind. These studies of nature in its refined aspects—the optimism in nature—touched a chord which vibrated with still higher harmonies. With him the progressive spirit was not only onward but upward toward the eminent domain of theologic aspect—theology the queen of all sciences.

No one realized his own limitations better than himself, but the ideal he ever held before him was fundamentally not subject to limitations, being neither more nor less than the Divine Personality who had said unto him, "I am the way, the truth and the life. Follow me." This dictum was to Joseph M. Wilson the most profound

yet comprehensive, from any point of view, ever uttered to humanity. He endeavored to lead the life thereby called for, and his works certainly do follow him, as he followed that ideal. He was kindness and love itself, even unto self-sacrifice—constant and enduring in good effort.

In one word, the life and career of Joseph M. Wilson manifested in well-balanced harmony the two conditions, material and spiritual, which call forth the best within a man as the wisdom of this age now perceives the truth in things as they are—viz., a sound, reasonable basis (scientific) for physical needs and intellectual life in all he studied, designed, advocated and executed—this being an up-to-date application of truth as natural science now recognizes it; also, a marked spiritual discernment of truth progressive as manifested in and through the religious consciousness of humanity under the ever-active ministry of the Holy Spirit of Truth in man himself—an inner life of good thoughts, giving utterance in good words, good deeds—an example of one who did follow in sincerity onward and upward toward the brightest and best.

Mr. Wilson married (1869) Sarah Dale Pettit, daughter of Judge Thomas McKean Pettit; great-granddaughter of Col. Charles Pettit and of Commodore Richard Dale, of Revolutionary memory, and of Chief Justice and Governor Thomas McKean, signer. He left one daughter, Mrs. John T. Gibson, of New York.

His domestic virtues were as beautiful, steadfast and altruistic as his professional life was admirable, sincere and progressive. He passed away in full belief of that higher existence in which there is "activity for all our powers, and power for all our activities."

March 23, 1903. HENRY PETTIT.

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OF THE

AMERICAN PHILOSOPHICAL SOCIETY

HELD AT PHILADELPHIA

FOR PROMOTING USEFUL KNOWLEDGE

VOL. XLII. MAY–DECEMBER, 1903. No. 174.

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OF THE

AMERICAN PHILOSOPHICAL SOCIETY

HELD AT PHILADELPHIA

FOR PROMOTING USEFUL KNOWLEDGE

VOL. XLII. MAY-DECEMBER, 1903. No. 174.

Stated Meeting, April 17, 1903.

President SMITH in the Chair.

Dr. Alfred Stengel and Prof. Edward Rhoads, newly elected members, were presented to the Chair, and took their seats in the Society.

Letters accepting membership were read from:

Edward E. Barnard, ScD., Williams Bay, Wis.
Carl Barus, Ph.D., Providence, R. I.
Franz Boas, Ph.D., New York.
Eric Doolittle, Philadelphia.
Basil Lanneau Gildersleeve, LL.D., Baltimore.
Francis Barton Gummere, Ph.D., Haverford, Pa.
George William Hill, LL.D., Nyack, N. Y.
Harmon N. Morse, Ph.D., Baltimore.
Edward Rhoads, Haverford, Pa.
Alfred Stengel, M.D., Philadelphia.
William Trelease, Sc.D., St. Louis.

The decease was announced of Mr. William V. McKean, at Philadelphia, on March 29, aged 82.

Dr. H. F. Keller presented a paper on "Recently Discovered Elements."

PROC. AMER. PHILOS. SOC. XLII. 174. S. PRINTED DEC. 14, 1903.

Stated Meeting, May 1, 1903.

President SMITH in the Chair.

Letters accepting membership were read from:
Arnold Hague, Washington.
Edward W. Morley, Ph.D., Cleveland.
Sir Henry E. Roscoe, F.R.S., London.
Prof. Hugo de Vries, Amsterdam.

An obituary notice of Joseph M. Wilson was read by Mr. Henry Pettit.

The decease of the following members was announced:
Theodore D. Rand, at Radnor, Pa., on April 24, æt. 66.
Prof. J. Willard Gibbs, at New Haven, on April 28, æt. 64.

Prof. Lightner Witmer read a paper on "The Modern Laboratory of Psychology," which was discussed by Prest. MacAlister, Prof. Haupt, Prof. Keasbey, Mr. Rosengarten and the President.

Stated Meeting, May 15, 1903.

President SMITH in the Chair.

Dr. Francis Barton Gummere and Mr. Eric Doolittle, newly elected members, were presented to the Chair and took their seats in the Society.

The following papers were presented:

On the "Languages of the New England Aborigines, New South Wales," by Mr. R. H. Mathews (see page 249).

"On Radio-Activity," by Prof. George F. Barker.

"On the Properties of the Field surrounding a Crookes Tube," by Prof. Arthur W. Goodspeed (see page 96).

Stated Meeting, October 2, 1903.

President SMITH in the Chair.

Letters accepting membership were read from:
William W. Campbell, Sc.D., Mt. Hamilton, Cal.
William Henry Howell, Ph.D., Baltimore.
Dr. Anton Dohrn, Naples.
Edwin Ray Lankester, LL.D., F.R.S., London.
Joseph John Thomson, D.Sc., F.R.S., Cambridge.

The decease of the following members was announced:
Dr. Thomas George Morton, at Cape May, on May 20, æt. 67
Prof. J. Peter Lesley, at Milton, Mass., on June 1, æt. 84.
Prof. Edward Rhoads, on July 4, æt. 29.
Prof. A. Radcliffe Grote, at Hildesheim, Germany, on September 12, æt. 62.
Prof. Edward North, at Clinton, N. Y., in September, æt. 83.

Prof. Angelo Heilprin made some remarks on "The Causation of Volcanic Phenomena, with New Researches in Martinique," which were discussed by Mr. Joseph Wharton, Prof. Amos P. Brown, Prof. Goodspeed and Mr. Bryant.

The following papers were read:

"The Existing Genera of the Trionychidæ," by Mr. O. P. Hay.

"Artificial Production of Crystallized Domeykite, Algodomite, Argentodomeykite and Stibiodomeykite," by Prof. George A. Koenig and Fred. Eugene Wright (see page 219).

"Some Names of Animals and Plants erroneously paired in Synonymy," by Marchese Antonio di Gregorio (see page 263).

ON THE EXISTING GENERA OF THE TRIONYCHIDÆ.

BY O. P. HAY.

(*Read October 2, 1903.*)

This subject was discussed in an interesting and instructive manner by Dr. George Baur in the PROCEEDINGS OF THE AMERICAN PHILOSOPHICAL SOCIETY, Vol. xxxi, p. 221, 1893. However, the present writer, on investigating the subject, has not been able to agree with Dr. Baur in all his conclusions, disagreeing with him partly regarding the types of some of the genera which he adopts, but especially on the value of some of these genera.

Dr. Baur was undoubtedly correct when he pointed out that the current employment of the name *Trionyx* for the majority of the living Trionychidæ is not justified, and that the genus has for its type *Testudo granosa* Schoepff, called *Trionyx punctata* by Baur, but recorded by Boulenger in his *Catalogue of the Chelonians*, p. 269, as *Emyda granosa*. This is in agreement with the views of Agassiz (*Cont. Nat. Hist, U. S.*, Vol. i, p. 395), who severely condemns the use of the name *Emyda* in this connection. Geoffroy's genus *Trionyx* was divided by Wagler in 1830. *Trionyx* was retained for *Testudo granosa*, while for most of the other species then known the new name *Aspidonectes* was adopted. The names of the species included under it are found in the second column of the table on opposite page. No type was indicated for the genus.

In 1831, Dr. J. E. Gray, in Appendix to Vol. ix of Griffith's *Cuvier's Animal Kingdom*, pp. 18, 19, and again in his *Synopsis Reptilium*, p. 49, applied the name *Emyda* (preoccupied) in place of Wagler's *Trionyx*, and *Trionyx* in place of Wagler's *Aspidonectes*. It is not necessary to add anything here to what Agassiz and Baur have said regarding this procedure, nor to do more than refer to Duméril and Bibron's proposal of the terms *Gymnopus* and *Cryptopus* to replace *Aspidonectes* and *Trionyx* respectively.

In 1836, Fitzinger (*Entwurf Syst. Anordnung Schildkr.*, pp. 119, 120, 127) further subdivided the species of soft-shelled tortoises. He made use of five sections, and these have since been employed as genera. These are *Trionyx*, *Aspidonectes*, *Platypeltis*, *Pelodiscus* and *Amyda*. The species enumerated under each of these are shown in the table already referred to. No types were indicated, but *granosa* was the only one named under *Trionyx*.

Table showing the date of the founding of the genera discussed in this paper, and the type of each.

A species marked with * became a type at that date.

Trionyx.	*Aspidonectes.*	*Platypeltis.*	*Pelodiscus.*	*Amyda.*	*Potamochelys.*
Geoffroy, 1809. subplanus. triunguis (ægyptiacus). cartilagineus (stellatus, javanicus) ferox (carinatus, georgicus). granosus (coromandelicus). euphraticus.	Wagler, 1830. triunguis (ægyptiacus). cartilagineus (javanicus). ferox (ferox, carinatus). spiniferus. muticus.	Fitzinger, 1836. *ferox (ferox, brongnartii).	Fitzinger, 1836. sinensis. triunguis (labiatus).	Fitzinger, 1836. subplana. mutica. euphratica.	Fitzinger, 1843. *cartilagineus (javanicus).
		Bonaparte, 1836. ferox.	Bonaparte, 1836. *sinehsis.	Bonaparte, 1836. *subplana.	
Wagler, 1830. *granosus (coromandelicus).	Fitzinger, 1836. triunguis (ægyptiacus). cartilagineus (javanicus). hurum. indicus.	Fitzinger, 1843. ferox.	Fitzinger, 1843. sinensis.	Fitzinger, 1843. subplana.	
		Tyrse.	*Dogania.*	*Chitra.*	*Isola.*
Fitzinger, 1836. granosus.		Gray, 1844. hurum (gangetica). gangetica (javanica). sinensis (perocellata). triunguis (nilotica). spinifera (argus). euphratica (raf-eht).	Gray, 1844. *subplana.	Gray, 1844. *indica.	Gray, 1873. *formosa (peguensis).
Bonaparte, 1836. granosus.	Bonaparte, 1836. *triunguis (ægyptiacus).				
Fitzinger, 1843. granosus.	Fitzinger, 1843. triunguis (ægyptiacus).				

Dr. Baur, in the paper referred to, concludes that inasmuch as the species *cartilagineus* (*javanicus*[1]) was fully figured by Fitzinger, it is the one to be regarded as the type of *Aspidonectes*. In coming to this conclusion he does not give due weight to what Fitzinger himself, in 1843, has done in the case; much less has he noted what Bonaparte had done still earlier. In Wiegmann's *Archiv für Naturgeschichte*, iv, 1, 1838, pp. 136–142, we find a paper by C. L. Bonaparte, entitled "Cheloniorum Tabula Analytica." In 1836 the same author issued at Rome a pamphlet of ten pages which bore the same title. This is understood to be a reprint from the *Giornale Arcadico*. I have not been able to see either the paper in the *Giornale* or the reprint, but Dr. Theodore Gill kindly informs me that the reprint made at Rome differs in only unimportant respects from the paper in the *Archiv für Naturgeschichte*. We find therefore, in this paper of 1836, that Bonaparte accepts two genera of Trionychidæ, *Amyda* and *Trionyx*, with four divisions under the former. With each of his names he mentions a single species, and these species, it seems to the present writer, must be regarded as the types of these subdivisions, all later treated as genera. Under *Trionyx* he mentions *Testudo granosa*; under *Aspidonectes*, *Trionyx triunguis* (*ægyptiacus*); under *Platypeltis*, *Testudo ferox*; under *Pelodiscus*, *Aspidonectes sinensis*, and under *Amyda*, *Trionyx subplanus*.

In 1843, Fitzinger (*Systema Reptilium*, p. 30) presented essentially the same arrangement of the Trionychidæ that Bonaparte had published in 1836. His two genera are *Trionyx* and *Aspidonectes*, the latter having under it five subdivisions, or subgenera. For *Trionyx*, *Aspidonectes*, *Platypeltis*, *Pelodiscus* and *Amyda*, he employed the same species as examples, or types, as did Bonaparte. For the newly proposed subdivision *Potamochelys* he used as type *P. cartilagineus* (*javanicus*). Dr. Baur made the objection that Fitzinger did not define the genus *Potamochelys*; but since the latter author refers to it a well-known species, it must be accepted as a valid genus, in case it really possesses generic characters. That is, technically it meets all the requirements of a generic name.

It may be noted here that Fitzinger's error of 1836, in distributing the species *triunguis*, under the names *ægyptiacus* and *labiatus*, to

[1] In the present paper the specific name now recognized is employed; if the author who is quoted employed a different name, this follows in parentheses.

both *Aspidonectes* and *Pelodiscus*, was not repeated in his work of 1843.

We may then, it appears to the writer, regard it as established that the type of the genus *Trionyx* is the species *granosus;* of *Aspidonectes*, the species *triunguis;* of *Platypeltis*, the species *ferox;* of *Pelodiscus*, the species *sinensis*, and of *Amyda*, the species *subplana*.

We must now consider how these determinations are to affect the work of subsequent writers, especially that of Gray, Agassiz, and Baur.

In 1844, Gray (*Cat. Tort., Croc.* and *Amphib.*, p. 46) established the new genera *Tyrse*, *Dogania* and *Chitra*, besides propagating his erroneous uses of the terms *Trionyx* and *Emyda*. The type of *Chitra* is *Trionyx indica* Gray, and this genus is yet recognized as a valid one. The type of *Dogania* is naturally the only species mentioned under it, *subplanus;* but this had already in 1836 been made by Bonaparte the type of *Amyda*, from which fact it follows that *Dogania* is a synonym of *Amyda*. Under *Tyrse* there were named six species, but no type was selected. In his later publications Gray dropped from *Tyrse* all the species originally included under it, except *triunguis* (*nilotica*). We must then suppose that he regarded this species as the type of the genus; but this was, as we have seen, the type of *Aspidonectes*, made so by Bonaparte in 1836. *Tyrse*, therefore, becomes a synonym of *Aspidonectes*.

Agassiz accepts *Trionyx ferox* as the type of *Platypeltis*. While rejecting *Pelodiscus* as a valid genus, he correctly states that it rests on *Trionyx sinensis* Wiegm. He does not say what he regards as the type of *Aspidonectes*, but he includes under it *Trionyx spiniferus*. *Amyda*, he states, has for its type LeSueur's *Trionyx muticus;* and he tells us that this generic name was vaguely applied by Fitzinger to one of his genera. As we have seen, no type was indicated for *Amyda* in 1836, but in 1843 Fitzinger names under the genus only the species *subplana*. There certainly was no vagueness in this procedure. Furthermore, Bonaparte had already in 1836 indicated the same species as the type of *Amyda*.

As already stated, Dr. Baur regarded the species *cartilagineus* as the type of *Aspidonectes* and *Trionyx muticus* as the type of *Amyda;* whereas Bonaparte in 1836 and Fitzinger in 1843 made *triunguis* (*ægyptiacus*) the type of the former, and *subplanus* as the type of the latter. Baur recognized *Testudo ferox* Schweigg. as the type of

Platypeltis, *Trionyx sinensis* as the type of *Pelodiscus*, and *T. subplanus* as the type of Gray's *Dogania*. Dr. Baur also recognized as valid genera *Cycloderma* Peters, with its type *C. frenatum*; *Cyclanorbis* Gray, with the type *Cryptopus senegalensis*; *Isola* Gray, with the type *Trionyx leithii*; *Chitra* Gray, with the type *Trionyx indicus*, and *Pelochelys*, with the type *P. cantorii*.

Leaving out of consideration the genera *Pelochelys*, *Chitra*, *Cycloderma* and *Cyclanorbis*, as being valid, and likewise invulnerable on other grounds, as well as the various genera founded since 1846, and cited by Boulenger as synonyms of his *Trionyx*, let us consider the content and value of the others.

In his classification of the Trionychidæ, Dr. Baur gave great weight to the amount of reduction of the posterior nares by the inner and posterior extension of the maxilla. To the present writer this character seems to be of little value. The two conditions of being "reduced" and of being "not reduced" can hardly be defined, and they are probably connected by every gradation. It is solely on this character, so far as we know, that he has separated generically his *Pelodiscus agassizii* and *Platypeltis erox* (*Amer. Naturalist*, xxii, p. 1121; PROC. AMER. PHILOS. SOC., xxxi, p. 217).

Trionyx, with *Testudo granosa* as type, must be regarded as a valid genus.

Aspidonectes Wagler, restricted by Bonaparte, 1836, and Fitzinger, 1843, with *Testudo triunguis* Forsk. as type, must be applied to the group designated by Boulenger I, B, 3 (*Cat. Chelonians*, p. 245), and to that included by Baur (PROC. AMER. PHILOS. SOC., xxxi, p. 220) under the name *Pelodiscus*, with the exception of his *P. agassizii*. In the same genus the present writer would include Boulenger's group I, B, 2, containing the species *cartilagineus*, *formosus* and *phayrei*. These were placed by Baur in the genus *Aspidonectes*, as this was limited by him; but did the group form a genus distinct from that whose type is *Testudo triunguis*, it ought to be called *Potamochelys*; since, as already stated, Fitzinger in 1843 made the species *cartilagineus* (*javanicus*) the type of this genus. This group differs from the preceding only in having "the alveolar surface of the lower jaw with a strong longitudinal symphysial ridge," a character which appears to the writer as insufficient. In the same genus must be placed *Trionyx subplanus* Geoffr. As

already said, Baur recognized it as the type of *Dogania;* but if it is a member of a genus distinct from *Aspidonectes*, it must be called *Amyda*, according to the systems of both Bonaparte and Fitzinger.

Platypeltis comes next, having as its type *Testudo ferox* Schneider. It will include all the American soft-shelled tortoises, except *Aspidonectes californiensis* (Rivers). The writer believes that this group is sufficiently characterized by the possession of only seven pairs of costal plates. The smooth or granular condition of the skin of the young is possibly a character of generic value. In this group must be included LeSueur's *Trionyx muticus*. There appear to be no characters which justify its separation as a distinct genus. Baur makes it the type of *Amyda*, following Agassiz. The only character given by Baur to distinguish it from *Platypeltis spiniferus*, for instance, is the separation of all the costals at the midline by means of neurals; whereas in the other American Trionychidæ the hindermost pair are in contact. This difference depends wholly on the greater or less development of the seventh neural plate; and this will almost certainly be found to vary in different species and in different individuals of the same species. Some importance has been attributed to the absence in *muticus* of the commonly occurring ridges, or papillæ, on the septum of the nares; but this character appears to the present writer to be of slight value. On similar characters the Trionychidæ might probably be divided into as many genera as there are species. If, however, *Trionyx muticus* is to form a distinct genus, a new generic name must be coined for it.

For Boulenger's group I, B, 1, Dr. Baur accepted Gray's generic name *Isola*, having, according to Baur's statement, *Trionyx leithii* as its type. This is, however, an obvious error. The genus was proposed by Gray in 1873 (*Proc. Zool. Soc. Lond.*, p. 51) for the reception of *Trionyx peguensis* Gray, and this is, according to Boulenger, a synonym of *Trionyx formosus*. *T. leithii* was afterward (*Ann. Mag. Nat. Hist.* [4], x, p. 157, 1873) referred to the same genus with some doubt. *Isola* is therefore a synonym of *Aspidonectes*, as recognized in the present paper.

The group of tortoises referred by Baur to *Isola* includes the species *gangeticus*, *hurum* and *leithii*. These species differ from those of *Aspidonectes*, especially in possessing two neural plates between the first costals. It appears to be worthy of generic rank. A search among the generic names which have been applied to the

members of the genus shows that none of them is available. I therefore propose the name ASPIDERETES (*ασπίς*, a shield, and *ερέτης*, a rower). The type is *Trionyx gangeticus* Cuvier, and the other living species will be *A. hurum* and *A. leithii*. It seems probable that a number of fossil forms must find their place in the genus.

Stated Meeting, October 16, 1903.

President SMITH in the Chair.

The following papers were presented:

"Evolution and Epigenesis—New Light on an Old Problem," by Prof. E. G. Conklin, which was discussed by Gen. Wistar.

"A Review of Parthenogenesis," by Mr. Everett F. Phillips, communicated by Prof. E. G. Conklin, which was discussed by Gen. Wistar.

A REVIEW OF PARTHENOGENESIS.[1]

BY EVERETT F. PHILLIPS,
HARRISON FELLOW IN ZOOLOGY.

(*Read October 16, 1903.*)

General Introduction.

In the great majority of cases the sex cells disintegrate unless they unite with the products of the opposite sex of the same species, but in many cases in the animal kingdom cells are given off from the germinal epithelium which, without fertilization, are able to undergo development, as is manifested by cell division. That these are true ova is evident from their origin, appearance, behavior and fate, and the only difference between these and eggs requiring fertilization is that they have in them the ability to divide mitotically without receiving the external stimulus given by the male sex cell. To this phenomenon the name Parthenogenesis is applied.

The importance of facts of this kind cannot be overestimated, especially from the standpoint of cytological investigation. The various ways in which these eggs behave during maturation and the sex relations connected with the different kinds of Parthenogenesis give us most valuable guides in our study and afford invaluable material toward the solution of that much debated problem—the determination of sex.

In view of the importance of the subject and the scattered condition of the literature, it has seemed desirable to give a brief summary of the most important work done, together with a literature list of all important papers. Most attention has been given to the case of the Honey Bee, since it was on this form that Dzierzon worked and especially since the most conflicting theories have been advanced concerning it. A somewhat lengthy discussion of this one case will make clearer what follows concerning other species, but it is hoped that this will not make it appear that I consider this the most important case, but that it is simply used as a basis for the later discussion.

The preparation of this paper was begun at the suggestion of Prof. E. G. Conklin to fill partially the need of some such review. I wish at this time to express my appreciation of the help

[1] Contribution from the Zoological Laboratory of the University of Pennsylvania.

and suggestions given me by Dr. Conklin all through the work. I wish also to state that I have referred constantly to the review of Taschenberg (1892) and especially to his long literature list. His paper is an excellent review up to the time of its publication.

Historical Sketch of the Theory.

The word Parthenogenesis (Greek *παρθένος*, a virgin, *γένεσις*, production) was first used by Owen[1] in the sense of Alternation of Generations.

In 1856, in his classic paper, "Wahre Parthenogenesis bei Schmetterlingen und Bienen," Carl Th. Ernst v. Siebold used the word in the sense of the development of eggs without fertilization, in which sense it has since been universally adopted. Previous to 1856 the phrase *lucina sine concubitu nulla* and similar terms were used in practically the same sense in which the word parthenogenesis is now used.

For the first observations on parthenogenetic development we must go back to Aristotle, as is true for the beginnings of so many lines of observation. This old Greek scientist recorded extensive observations on the Honey Bee which will be referred to in another place.

The next writer who gave any intimation of a belief in such phenomena was Goedart (1667) who succeeded in raising larvæ from eggs laid by an unfertilized female of *Orgyia gonostigma*. After that Leenwenhoek (1695), Blancard (1696), Albrecht (1706) and Réamur (1737 and 1741) recorded somewhat similar results.

In 1745 Bonnet, of *emboîtement* fame, described, rather fully, parthenogenetic development in plant lice. Oscar Hertwig, in his "Historical Account of Embryology," in the *Entwicklungslehre*, speaks of Bonnet's work in the strongest terms and does not hesitate to designate it as marking one of the milestones in the history of embryology.

Just one hundred years after this, Dzierzon (1845) announced his theory on the parthenogenetic development of the drone eggs of the common bee, *Apis mellifica*, which will be treated more fully in a later section. During this period of one hundred years a

[1] V. v. Prosch, in 1851, in *Om Parthenogenesis og Generationsvexel, et Bidrag til Generationstaeren* (Kjobenhavn, Trijkt hos J. C. Scharling), used the word in the same sense.

number of papers appeared in which the development of unfertilized eggs was described, but the importance of the observations was not recognized fully until after Dzierzon published his first paper. This paper, published in a bee journal, may well be looked on as the starting-point of the Theory of Parthenogenesis, since it started a very important discussion and marks the beginning of a host of work along similar lines.

The most important papers of the period between 1745 and 1845 will be found in the literature list at the end of this paper. It does not seem desirable to go into a detailed account of these earlier papers since, while they are valuable, the greatest additions to our knowledge of these phenomena have been made since the time named.

As stated on a preceding page, more attention has been paid to the parthenogenesis of the Honey Bee, in the preparation of this paper, than to any other form. A full statement of the present state of our knowledge of the phenomena in this species will make clearer what follows concerning other species.

Theories on the Honey Bee Previous to 1845.

Before discussing the various theories and experiments on the parthenogenetic development of the drone eggs of the common bee, it may be of interest, from the historical standpoint, to review briefly the various theories put forth previous to 1845 which were used to explain the peculiar phenomena observed in the hive in regard to the sex of the bees. Since the bee is of economic value it has been the object of much investigation for centuries, and for this reason the peculiarities of its development have long been known.

Aristotle, in his *Historia animalium*, wrote: "All persons are not agreed as to the generation of bees, for some say that they neither produce young nor have sexual intercourse; but that they bring their young from other sources. . . . Other persons affirm that they collect the young of the drones from any of the substances we have named (flowers of the honeysuckle, reed or olive), but that the rulers (queens) produce the young of the bees (workers). . . . Unless the ruler (queen) is present drones only are produced. Others affirm that they have sexual intercourse, and that the drones are males and the bees females." In his *De generatione animalium* he wrote: "The drones develop in a queenless

stock" and "The bees produce drones without copulation." Here we get a rather clear statement of what was rediscovered centuries later.

Huish gives an account of other theories advanced, and a large part of the information from which these summaries of the earlier work were made is from his paper.

1. Schirach says that the hive consists of three kinds of bees: (1) queens, the mother of the hive, (2) drones or males, and (3) workers, a middle sex with greater affinity to the queen but destitute of procreating powers. The parts which belong to the queen lay concealed in imperceptible minuteness, and just as soon as they receive the necessary space for their expansion, increase takes place in size and a queen is developed. Drones from fertile workers and queens arise from false or corrupted eggs, to which the name "abortion" is applied. Some of the opponents of Schirach held that all workers lay eggs, the view being based on the fact that in queenless hives drones are produced by the "fertile workers."

2. Herold was one of the greatest opponents of Schirach, maintaining that the queen copulates with a male worker, producing male and female workers. The true workers, male workers, perform their duties outside the hive, collect honey and pollen and copulate with the queen and female workers which remain inside the hive. The female workers lay eggs (fertilized by male workers) which produce drones of no sex whatever. This was at once proven false by an anatomical examination showing that the drones are males. The hive was then considered as an Amazon republic with drones raised to the rank of males or husbands, a view that had many supporters up to the time of Heinmetz.

3. Heinmetz proposed a double genealogical tree for the bee family, symmetrically for both the male and female lines. (1) The queen as the great mother bee copulates with a male worker and lays eggs producing insects like their sire (male workers). If laid in large cells they produce great male bees, if the rudiments of a great male exists in the egg.[1] "But as only small male workers are the issue, although they may be bred in large cells, the conclusion must be drawn that in these male eggs the rudiment was only existing for small workers and that from these no great male bees are pro-

[1] Quotation from Huish. See former reference.

duced." (2) The queen also lays eggs producing females which resemble the queen or are female workers or mothers of the drones. The working bees are partly male and partly female and are derived from the queen. On the other hand the drones are from a mother drone, as follows: A mother drone copulates with a great male drone and lays only drone eggs which develop as small drones or as great drones (like their sire). Needless to say, a theory of this kind had many opponents.

4. Voigt and Lucas. These men separately maintained that the queen is the mother of all the bees, laying in six months of the year an almost incredible number of fertilized eggs, from which in twenty to twenty-four days are produced common workers which are both male and female. The males *by their mouths* fructify not only the queen but common female workers or mother drones, and from eggs laid by the latter in May and June drones are developed. This fructifying or vivification of all these eggs is performed and executed by the principle of life or by the animating creative spiritual power, *aura seminalis*, contained in the spittle, the process of which is so very visible in the frequent application of the proboscis of the common male bees to that of the queen. This theory was based on the facts that workers and queens can compose a perfect hive without adding drones and that workers produce drones.

5. Haumann maintained that the queen is the only mother of her like and of workers and drones. The bees (workers) are nurses and co-operate in breeding, and without them the eggs prove abortive. In the small cells the sex property of female eggs is lost and the egg becomes a common bee, but in a royal cell a queen or fertile mother, and in drone cells a spurious mother drone. The male eggs in common cells become bees devoid of sex, and in drone cells a male or drone. Hummel attacked this most violently on the principle that it is at variance with every analogy of nature to invest an insect with the power of altering the sex character of an egg after laying, and impart to it a power which did not belong to it in its original nature. From Hummel's argument was founded one of the chief objections to the hypothesis advanced by Huber, that a common bee is possessed of the power of generating a queen from a common egg.

6. Strube held that the queen with a double-branched ovarium lays male and female eggs. The male eggs are placed in small cells and become male workers. The female eggs become queens

in queen cells or degraded queens. The remaining workers are those which can breed only drones; they are fertilized by the male workers and not by drones. The eggs of drones of May are laid by degraded queens. The ovaria of these queens cannot develop in the small cells and are weakened. During honey flow these degraded queens lay eggs. The eggs from which early drones arise are laid in the autumn and are outside the heat of the hive in winter, developing in spring. It is only when there is a deficiency of male workers that the queen is fertilized by a drone.

Habits of the Bee.

In order to appreciate fully the experimental work done on the subject of the parthenogenetic development of the male bee, it is necessary to know something of the habits of the different members of the hive or colony. The habits of no insect are better known to zoologists, but a very brief statement may not be out of place here, although necessarily incomplete.[1]

At the age of about five days the queen takes what is commonly spoken of as her "marriage flight," flying from the hive to meet a drone. She returns in about half an hour with the organs of the male generally hanging to her; the copulation taking place on the wing and the male being killed in the operation. Before the marriage flight the spermatheca is filled with a clear fluid and afterward it contains a white liquid, the seminal fluid, the number of spermatozoa having been estimated at several millions. Since a queen lays during her lifetime, averaging three or four years, a total of possibly 500,000 eggs, it will be seen that the apparatus for preserving sperm cells is very perfect. The spermatheca opens by a tube into the oviduct, the tube being surrounded by highly enervated muscles and accompanied by accessory glands which probably nourish the spermatozoa. These muscles must contract during the laying of a

[1] The facts here given regarding bees are gathered from various sources and from personal observation, and only such facts are here introduced as seem necessary to a better understanding of the discussion following. For more detailed accounts any book on apiculture may be consulted, of which the following are some of the well known examples:

Root, *A B C of Bee Culture*, Medina, O.

Cook, *Manual of the Apiary*, Lansing, Mich.

Benton, *The Honey Bee*, U. S. Dept. of Agriculture, Washington, D. C.

drone egg, so that no sperm cell can reach the oviduct to fertilize the egg.[1]

During the active season the queen can under stress of circumstances lay eggs at the rate of four a minute, although generally much slower, and in twenty-four hours can lay over 4000 eggs, the total weight of which is more than the weight of her own body. The eggs are laid at the bottom of the cells, the abdomen of the queen being put into the cell during the oviposition, and the eggs are attached to the middle point of the base of the cell by the end opposite the micropyle. In the hive the eggs are laid in what are known as brood cells, generally situated near the middle of the hive, these cells being used for the storing of honey when not used for larvæ. The cells from which the workers hatch are about one-fifth of an inch across, while those from which drones hatch measure about one-fourth inch; these being spoken of as worker and drone cells respectively. The royal or queen cells, in which queens develop, are shaped like an acorn and occupy about the space of three ordinary cells, these being built naturally only when the hive is queenless, when the queen is to be superseded by another on account of her age, or at the swarming season when the hive is to be divided. The queen passes quickly from one cell to another, laying in each an egg which almost invariably develops according to the size of the cell. This necessitates a very fine manipulation of the entrance of the spermatheca or seminal receptacle, as the sex is dependent upon whether a spermatozoon is allowed to escape or not.

Various theories have been advanced to explain the power of the queen to control the escape of the spermatozoa since we cannot believe that it is a conscious act, in spite of statements to that effect. A very plausible one is that the difference in the size of cells causes a difference in the pressure of the abdomen, and by a reflex nervous action, of the nature of which we know nothing, the muscles are contracted when the abdomen is put into a drone cell. Kückenmeister was probably the first to advance this theory. In opposition to this Cook (1881) and many others cite the fact that queens lay fertile eggs in cells where the walls have not yet been built up, and in such cases pressure on the abdomen could play no part. We have not as yet been able to account for the nearly

[1] For a description of these parts of the queen see Cheshire, F. R. (1886).

infallible ability of the queen to determine the sex of each egg. Probably queens never lay eggs in queen cells, but when a new queen is desired the workers build out a queen cell over a cell containing a very young worker larva (less than one day old). At any rate, this is the general method of procedure, although I have seen a drone-laying queen lay eggs in a partly built queen cell.

When a hive becomes hopelessly queenless it frequently happens that certain of the workers begin to lay eggs, which of course produce nothing but drones since a worker never copulates with a drone. These are called Fertile or Laying Workers, and are far more easily produced in the races of bees found in Eastern Asia than in the Italian bees.

The species *Apis mellifica* is divided into several races, the principal differences being in the coloration of the segments of the abdomen, although the instincts differ slightly, especially as regards the production of queens. The two races on which experiments on parthenogenesis have been performed are the Germans and Italians. The former are almost entirely black, while the latter have bands of yellow on the abdomen, three to five in number, or occasionally six. This difference has been used as a means of determining the truth of the parthenogenetic development of the males.

The Theory of Dzierzon.

The parthenogenetic development of the male eggs of the bee, *Apis mellifica*, was first observed by Johannes Dzierzon, a priest of Karlsmarkt, Germany. He was a bee-keeper of many years' experience and a good observer. The theory was first announced in the *Eichstadt Bienenzeitung* in 1845, and in 1852 was published in book form. His arguments were briefly as follows:

(1) [1] A queen to be of any value must be fertilized by a drone. This takes place on the wing, high in the air. Drone eggs are not fertilized, but worker and queen eggs always are. "[2] In copulation the ovaries are not fecundated, but the seminal receptacle, that little

[1] The results of his investigations and his conclusions appeared in the *Eichstadt Bienenzeitung* and other journals, most of which were not accessible in the preparation of this paper. They were recorded in a very large number of short papers and it does not seem desirable to refer to all of them at this time. A complete list of the writings of Dzierzon can be found in *Bibliotheca Zoologica*, II, O. Taschenberg, to which the reader is referred.

[2] The quotations from Dzierzon are translations made by Lowe (1867).

vesicle or knot which in the young queen is filled with watery moisture, is saturated with semen, after which it is more clearly distinguishable from its white color." The supply of semen is enough for a lifetime. No clipped queen can be fertilized, as copulation never takes place in the hive. "The power of the fertile queen, accordingly, to lay worker or drone eggs at pleasure is rendered very easy of explanation by the fact that the drone eggs require no impregnation, but bring the germ of life with them out of the ovary; whilst otherwise it would be inexplicable and incredible. Thus the queen has it in her power to deposit an egg just as it comes from the ovary, and as the unfecundated mothers lay it; or by the action of the seminal receptacle, past which it must glide, to invest it with a higher degree, a higher potency, of fertility and awaken in it the germ of a more perfect being, namely a queen or a worker bee."

(2) The most important point in the theory is that "All eggs which come to maturity in the two ovaries of the queen bee are only of one and the same kind, which when they are laid without coming in contact with the male semen become developed into male bees, but on the contrary when they are fertilized by male semen produce female bees."

This, as v. Siebold expresses it, "strikes at the root of and completely abolishes the time-honored physiological law that an egg which is to be developed into a male or female individual must always be fertilized by male semen." Dzierzon refers to Riem, a French naturalist, for the fact that fertile workers lay only drone eggs (a fact now well known from many sources), and Mme. Jurin found on anatomical investigation that these fertile workers were queens with the spermatheca aborted and the ovaries not fully developed. Dzierzon also asserted that a queen must be able to lay either drone or worker eggs *at will*.

v. Siebold wrote: "We might beforehand expect that by the copulation of a unicolorous black-brown German and reddish-brown Italian bee the mixture of the two races would only be expressed in the hybrid females or workers but not in the drones, which are produced from unfecundated eggs. They must remain purely German or purely Italian according as the queen selected for the production of hybrids belongs to the German or Italian race." In 1854 Dzierzon wrote: "Continued observations of the hybrid hives also must be no less adapted to raise the veil, more and more to penetrate into the obscurity and finally bring the mysterious

truth to light. If the drone egg does not require fertilization, Italian mothers must always produce Italian drones and German mothers, German drones, even when they have been fertilized by drones of another race." His faith in this proposition was so strong that when in a few years he found one case in which it did not seem to hold good he gave up his theory, just when it was becoming generally accepted, and as an explanation took up the old exploded theory of Swammerdam of the vivifying action of an *aura seminalis*. Either the experiments of Count v. Berlepsch[1] or the work of v. Siebold reconverted him, for in 1861 he reiterated his belief in his theory.

Experiments and Later Investigation on Bees.

Owing to the fact that the phenomena connected with parthenogenetic development of the Drone Bee are so striking, even to a person not used to scientific methods of investigation, many experiments have been tried to test the Theory of Dzierzon. Journals devoted to Bee Culture as well as more strictly scientific publications have recorded a large number of experiments, of which but a few can be mentioned here.

Lowe (1867), after several years of experimenting with hybrid hives, denied the truth of Dzierzon's Theory. With Italian queens fertilized by common black drones he could get no definite results, but with Egyptian queens fertilized by black bees he obtained many drones which appeared to have characteristics of the male parent. His work was not so carefully recorded as was that of Perez which will be mentioned later.

Landois (1867) put worker eggs in drone cells and drones were produced, and *vice versa*. This he did many times and his results were verified by the presence of the little piece of wax, to which the eggs had been attached, sticking to the cocoons. He in every case cut out a little piece of the wax at the base of the cell and stuck this with the egg attached into the new cell, so that the egg was not injured by the transfer. His earlier experiments were not successful, due to imperfect manipulation. His conclusion then was that sex in the bee is determined by the food given the larva

[1] v. Berlepsch upheld the theory in a large number of papers in the *Eichstadt Bienenzeitung*. For a list of his writings see *Bibliotheca Zoologica*, O. Taschenberg, Zweiter Band, pp. 252–3.

and not by fertilization. It is known that after the eggs are hatched, at about the third day, the workers pour into the cell a food paste for the nourishment of the larva. Great quantities of this are eaten for six days and then the workers cap the cell, and in ten or eleven days the bee in its adult form comes out. The cap put over the smaller worker cells is flat; that over drone cells, arched. v. Siebold (1868), in answer to this theory, points out that sex is differentiated early in insect larvæ. Herold, for *Pieris rapæ*, was able to tell sex early. On the other hand Meyer did not see this in caterpillars only a few days old. Weismann, in *Musca vomitoria* and *Sarcophaga carnaria*, confirms Herold, but is not so sure in the case of *Corethra plumicornis*. Leuckart (1865) found first traces of external genitalia on the sixth day in Apis. v. Siebold insists that all embryos (queens included) up to the sixth day get food paste (digested chyle paste). The queens continue to get this, and from that time on the workers and drones get undigested honey and pollen. The food of the drones and workers is therefore the same. Landois thinks the drones of unfertile queens and of fertile workers are due to scanty nourishment or weak larvæ, for in *Vanessa urticæ* only males are produced if badly fed. v. Siebold (1871) does not find this true in *Polistes gallica*, for in the spring, when food is scarce, workers are produced; and Cuenot (1899) denies the truth of all such statements which make the sex depend upon nutrition.

Sanson and Bastian (1868) attempted to repeat the experiments of Landois, but in every case when the egg was put in a different cell the workers in the hive carried it outside. Never in a single case was the egg allowed to develop and they were therefore led to deny the experiments of Landois. The reason for their failure, as pointed out later by Landois, was imperfect manipulation. They cut out the entire bottom of the cell and stuck it in place by melting the edge with a hot needle, and this made such a bad job as compared with the work of the workers that they cleaned it out. Sanson (1868), in opposition to Landois, also cites cases of the production of drones in worker cells. This is now well known, as is also the converse, and this fact alone is enough to overthrow all of the work of Landois.

Perez (1878) put a pure Italian queen fertilized by a French drone into a hive with pure French workers and no drones. Later in the season he collected and examined carefully three hundred

"Pure Italian queens vary all the way from a jet black to a bright yellow. We had one daughter from an imported Italian that was very black; but her bees (workers) were uniformly well marked and showed all the characteristics of pure Italians. Some of the queen daughters of the imported queen are quite yellow and some quite dark. Any one who attempts to judge of the purity of drones or queens by their markings has much to learn about bees."

I put a great deal of confidence in the statements of Mr. Root, since he is thoroughly informed in things relating to bees from a practical standpoint and is a man of high standing in his line of work. We must conclude then that in the honey bee we have a case in which certain racial characters are constant only in the abortive females, although they do not normally enter into the reproduction of the species. Since these markings are not a constant character, even in pure drones, any attempt to use them as tests of hybridism is not warranted.

A comparatively large number of cases have been recorded of hermaphroditic or androgynous bees. This fact was long since noticed by Lucas, more recently by Doenhoff, Menzel and Engster, and in 1864–5 by v. Siebold and Leuckart. There is a mixture of male and female characters, varying in different individuals, in both internal and external organs. Very often on each side of the body a few testicular cords and a few ovarian tubes, a well-developed male copulatory apparatus and a sting are developed, or one side of the body may be entirely male, the other side female. According to Leuckart all these must be regarded as workers with some male characteristics. The explanation offered is that fertilization did not take place here until after the male characters had become too well fixed to be thrown aside by female characteristics.

Boveri (1901) in a late paper suggests that such cases are due to the late fertilization of the egg, after mitosis has commenced, and as a result part of the cells have paternal characters and are therefore female, while the unfertilized portion remains male. This would, of course, easily explain the great differences in hemaphroditic bees.

There are numerous cases on record of queens which have taken their marriage flights and on their return to the hive, and during the rest of their lives, have laid eggs which never develop. The opponents of the theory of parthenogenesis eagerly take up a case

of this kind, claiming that for some reason the queen has not been ertilized and that on this account her eggs will not develop. v. Berlepsch was probably one of the first to make any observations on this line, and his conclusion was that it was due to some pathological condition of the queen.

Claus and v. Siebold (1873) took up this subject and carefully studied several cases that came to their notice. One of the cases was that of an Italian queen, born May 15, began to lay June 15 and continued until October 5, when she was killed. Her eggs did not hatch and an examination showed that her oviducts were normal, spermatozoa present in the spermatheca, but the ovarian tubes were degenerate. The conclusion, from this and other cases examined, was that all such cases of sterile queens are probably due to some irregularity in the formation of the ovum, and especially of the vitellus. Leuckart (1875) reports other cases examined and corroborates Claus and v. Siebold.

Of the opponents of the theory of Dzierzon, none perhaps are as radical as Ulivi (1874–82). His views were briefly as follows: Queens are usually fertilized in the hive, and he claims to have witnessed the act of copulation several times. The spermatheca, on the return from the so-called "marriage flight," is clear and contains no spermatozoa, as was demonstrated by numerous examinations. The marriage flight is explained as being merely for exercise. Drones are not mutilated in copulation, and on examination the white appendage which is always seen on the queen on her return from the marriage flight is found to be excreta. Every egg, male or female, is fertilized. Queens that were never allowed to fly (their wings being clipped) were put in hives without drones and laid no egg or eggs that did not hatch. Every queen whose spermatheca is distended has been fertilized. None of the eggs of a queen that has never met a drone will hatch. There is no such thing as a fertile worker. Fertilized eggs will keep through the winter and hatch out in the spring. He also claims that there can be no true parthenogenesis when a fertile copulation is admitted. The effect of the spermatic threads does not consist of a simple excitement of the supposed vital germ preëxisting in the egg, but of a real infusion of the absolute principle of life. No transformation of sex can be effected by spermatic injection. It need scarcely be added that such views have found no supporters.

For the past two or three years Dickel has been advancing a new

theory in regard to the determination of sex in the bee and he has some supporters, although the number of these seems to be decreasing. His views are briefly the following: Eggs laid by unfertilized queens or fertile workers produce drones, but these differ from the drones of a colony with a fertile queen. The egg before fertilization contains only male elements, the sperm cell only female, and after union of the two these are equally balanced. A fertile queen can lay only fertilized eggs since she cannot withhold sperm cells. The workers, in crawling over the brood cells just after the eggs are laid, pour out a secretion which penetrates the chorion of the egg. The wax, in the formation of brood cells, is kneaded in the mouths of workers and is impregnated from the salivary glands with a secretion characteristic of drone or worker cells, and this determines the kind of cell made and consequently the nature of the secretion poured out over the egg when laid. The two sexes are equally balanced in the newly-laid egg and the workers pour out a secretion from one of two glands in the head, the secretion from one causing the egg to develop into a male; of the other, into a female. The secretion of the "salivary" gland of the workers is comparable to a sexual act and probably produces similar emotions. Sex cannot be determined by mere size of cell or by food. These glands have been observed in the queen in a rudimentary state and in wasps. It is further claimed that experiments (performed by Dickel himself) on hybrid hives have clearly shown paternal characteristics in male offspring.

Weismann and his students, Petrunkewitsch and Paulcke, have pointed out the errors in this theory and, from work of their own, strongly reaffirm the view of Dzierzon, that sex is here determined by fertilization.

Other Cases of Parthenogenesis.

Classification.—Parthenogenetic development manifests itself in a variety of ways and many synonymous terms have been applied to the different kinds of parthenogenesis. The following classification will serve to make clear the relations of the different phenomena to one another and to show the synonymous terms used:

Parthenogenesis (Agamogenesis).

1. Partial.

   Development to early cleavage or larva.

   e.g., Vertebrates (?) and Echinoderms.

2. Complete—to adult condition.
   (*a*) Occasional—exceptional—Tychoparthenogenesis (Henneguy).
   e.g., *Bombyx mori.*
   (*b*) Normal—Isoparthenogenesis (Hatschek).
   (1) No Alternation of Generations.
   e.g., *Apis*, *Nematus.*
   (2) Alternation of Sexual and Parthenogenetic Generations—Heteroparthenogenesis (Hatschek), Heterogeny (Leuckart), Pseudoparthenogenesis (Spencer).
   e.g., *Aphis*, *Daphnia.*

The following classification of Complete Parthenogenesis is based on the sex of the resulting individuals:

1. Homoparthenogenesis (Henneguy), Complete Parthenogenesis (Spencer).
   One sex only produced from unfertilized eggs.
   (*a*) Arrenotoky (Leuckart), Androgenetic (Breyer).
   Males produced. *e.g.*, *Apis.*
   (*b*) Thelytoky (v. Siebold), Gynogenetic (Breyer).
   Females produced. *e.g.*, *Psyche.*
2. Heteroparthenogenesis (Henneguy), Mixed Parthenogenesis (Stein).
   Amphoterotoky (Taschenberg), Amphotoky (Lankester).
   Both sexes produced parthenogenetically. *e.g.*, Aphidæ.

An Alternation of Generations often accompanies parthenogenetic development, and in the literature considerable confusion occurs by a mixing of the terms. For this reason the following classification is given so that the occurrence of Parthenogenesis in relation to Alternation of Generations may be made clear:

Alternation of Generations (*Metagenesis* Owen).

1. Sexual Generation alternating with a Budding Generation.
   (*a*) Buds remain attached to form colonies.
   e.g., Medusæ and Polyps.
   (*b*) Buds separate.
   e.g., *Salpa.*
2. Sexual Generation alternating with Parthenogenetic Generation.
   Heteroparthenogenesis (Hatschek), Heterogeny (Leuckart).

3. Two Sexual Generations differing in form—Alloigony (Leuckart).
   (*a*) One free generation, one hermaphroditic and parasitic.
   e.g., *Rhabdonema*, *Allantonema*.
   (*b*) Seasonal Dimorphism.
   e.g., *Lophyrus pini*.

Pædogenesis or the parthenogenetic reproduction by larval forms is frequently met with (*e.g.*, Diptera). This term was introduced by v. Baer (1864), but unfortunately it has since been applied by Seidlitz (1872), Dilling (1880) and others to all cases of sexually mature larvæ, even though the reproduction be truly sexual. Thus they would include under this term the reproduction of Axolotl and of Gyrodactylus. v. Siebold (1869) used the term pædogenesis for the reproduction of the Strepsiptera, but in this case the sexually mature female is simply a degenerate adult and not a larval form as v. Siebold supposed, and the reproduction is sexual as far as the evidence at present goes. To aid in the clearing up of this confusion of terms, Taschenberg (1892) suggests the term Proiogony for all cases of sexually mature larvæ, so that the word Pædogenesis can be used in its original and proper meaning. Chun (1892) uses the term Dissogonie for cases like those found by him in *Cydippe*, where the same individual at different stages of development is sexually mature, and these stages are separated by a metamorphosis.

The word Pseudoparthenogenesis has been applied by some writers to cases in which the eggs are fertilized from a seminal receptacle (*e.g.*, female eggs of *Apis*), and in which copulation does not take place for each egg. The use of such a word is unfortunate since it implies that there is a similarity to parthenogenesis, while there is really a very fundamental difference.

Insecta.

Hymenoptera.—Besides the case of the Honey Bee referred to at some length on preceding pages, numerous other cases of parthenogenesis occur among the Hymenoptera.

Tenthredinidæ.—The first case described in this family was that of *Nematus ventricosus* (=*N. ribesii*) by Robert Thom (1820) who wrote: "The insect is male and female, but the ova of the female produce caterpillars, even when the male and female flies are kept separate. How long this offspring would continue to

breed has not been ascertained. . . . There is some reason to suspect that there is a connection between male and female caterpillars, for I have frequently observed them twisted together for some time after they have ceased eating, and a little before they cast their skins to go into the pupa state." This same form was investigated by Kessler (1866) and especially by v. Siebold (1871). Other papers on this family are those of Cameron (1885), Fletcher, (1880), v. Stein (1881–83) and Brischke (1887). Taschenberg (1892) gives a long list of members of this family for which parthenogenetic development has been recorded. The various members of the group afford examples of Arrenotoky, Thelytoky and Amphoterotoky.

Cynipidæ.—In this family many species are known only from females, males being entirely absent or very rare. Leon Dufour (1841) found no males in two hundred individuals of *Diplolepis gallæ tinctoriæ* collected, and Hartig (1843) no males in nine thousand examples of *Cynips divisa*. Osten-Sacken (1861) attempted to explain this by claiming that the males live in different galls from the females and are not recognized as the same species. Such a dimorphism is known for some Cynipidæ and it is probably true for many more. Taschenberg (1892) gives a list of nineteen cases in which males and females have been described as different genera and are now known to be but cases of sexual dimorphism. *Cynips quercus-cærculata* (Osten-Sacken) which produces a large gall in the autumn, in the spring of the next year lays eggs which produce galls of another form, originally named *C. q. spongifica*. The autumn brood of this *Cynips* consists of parthenogenetic females, while the spring brood is of both males and females.

Neuroterus lenticularis produces galls of a certain form on the under side of oak-leaves and the flies appear in the early spring. These deposit their eggs on the buds of the oak which produce galls unlike those of the autumn and the fly, of both sexes, which emerges from the second gall has been referred to a separate genus (*Spathegaster baccarum*). This in turn lays eggs which produce the original form of *Neuroterus*, all females.

In the families of Ants and the family Vespidæ parthenogenesis similar to that of *Apis* is very common, as is also true for other species of the family Apidæ. The best known cases are those investigated by v. Siebold (1870–71), *Vespa germanica* and *Polistes gallica*.

Andrenidæ.—In *Halictus*, according to Fabre (1880), a mixed brood results from the development of the unfertilized eggs, Amphoterotoky. Cf. Perez (1895).

Ichneumonidæ.—v. Siebold (1884) describes Thelytoky for *Paniscus glaucopterus*.

Chalcididæ.—Adler (1881) describes an alternation of generations and probable Arrenotoky for *Pteromalus puparum*.

COLEOPTERA.—Few cases of parthenogenesis are recorded for this sub-order, Osborne (1879–81) and Jobert (1882) being the only observers who record such phenomena. The cases recorded are *Eumolpus* (*Adoxus*) *vitis* and *Gastrophysa raphani* (*Gastroidea viridula*). Osborne considered parthenogenesis in *G. raphani* to be as frequent as in *Nematus ribesii*, while Jobert suggests that the form studied by him (*Adoxus*) is hermaphroditic. v. Siebold (1869) described pædogenesis for the Strepsiptera, the females of which are wingless and worm-like with a flattened triangular head and live in the abdomen of bees and wasps. The female is viviparous, producing hundreds of young, but is not a larval form at the time of reproduction, and there is no evidence that fertilization does not take place.

LEPIDOPTERA.—In *Bombyx mori* occasional parthenogenesis has been observed. Constans de Castellet (1795) first recorded this, and it was confirmed by Herold (1838) and Leuckart (1855). v. Siebold (1856) and a pupil Schmid got both sexes from unfertilized eggs. Verson (1873) showed that reproduction in this case is generally sexual and (1888) claimed that parthenogenetic development for this species is usually partial. Tichomiroff (1886–91) produced partial parthenogenesis in this form by mechanical excitement (1886) and by putting the eggs in 65 per cent. sulphuric acid for two and one-half minutes (1889). Nussbaum (1898) found that two per cent. of the eleven hundred unfertilized eggs examined showed segmentation but never hatched, and in similar observations on the eggs of *Parthesia* and *Liparis* he did not get cleavage in any case.

In *Solenobia triquetrella*, *S. lichenella*, and *Psyche helix* true Thelytoky occurs and we have a succession of parthenogenetic females, and only occasionally in *P. helix* is a male produced.[1] Much of the early work on parthenogenesis was done on Lepidop-

[1] Described by Claus, 1866. No males are known for Solenobia.

tera, some of the workers being Réaumur (1738), Pallas (1767), Degeer (1771), Kühn (1775), Schiffermüller (1776), Sehrank (1776 and 1802), Scriba (1790) and Reutti (1810). v. Siebold was at first (1849) inclined to doubt the existence of parthenogenesis in these species, but in 1856 published the results of elaborate experiments in which it was fully proven. Speyer (1847), Wocke (1853) and Reutti (1853) reached similar conclusions, and Leuckart (1858) examined the females of *Solenobia* and found no spermatozoa in the seminal receptacle, although there was a micropyle on the egg. Hartmann (1871) raised many successive generations of individuals parthenogenetically.

HEMIPTERA.—The first to investigate the reproduction of Aphids was Leeuwenhoek (1695). He found that the young are produced vivaparously and that there are few males, and Réamur (1737) from like observations, on theoretical grounds, held that they are protandric. Bonnet (1745), who generally gets the credit of having first observed the reproduction of the group, raised nine generations of viviparous females in two and one-half months in summer, and in the fall males appeared which copulated with the females, and eggs were laid which hatched out in the following year. Degeer (1773) worked on *Lachnus pini* and *Aphis rosæ*, and concluded that sexual individuals could be entirely done away with by keeping the insects protected from cold, and in this he was confirmed by Kybér (1815), who raised fifty successive generations of viviparous individuals in four years. Most of these earlier workers thought that the viviparous individuals were larval forms, which would afterward develop into the oviparous individuals.

Similar experiments led Duvau (1825) to believe that the oviparous and viviparous individuals are entirely distinct and that they never have the power of reproducing in both ways, and later Morren (1836), for *Aphis persicæ*; Ratzeburg (1844), for *Aphis oblonga*, and Newport (1847), for *Aphis rosæ*, came to similar conclusions.

Dufour (1841) repeated the experiments of Bonnet and referred the reproduction of *Diplolepis gallæ tinctoriæ* to "spontaneous or equivocal generation, in which impregnation is in no way concerned." Morren (1836) also believed in this spontaneous generation and thought that Aphids are developed in the body of the virgin parent: "Comme chez quelques entozoaires par individualisation d'un tissu precédément organise." [1]

[1] Page 90, *loc. cit.*

v. Siebold (1839) examined the viviparous and oviparous females and found that there is no appreciable difference between the ovaria of the two, but that the former lack a receptaculum seminis, and are, therefore, incapable of copulation. In the former point he was confirmed by Owen (1849), but not by Steenstrup (1842), who insisted that the viviparous individuals do not have ovaries but a well-developed uterus; to these he gave the name "Ammen" or nurses.

A most important step in advance was made by Steenstrup (1842) when he introduced the idea of an Alternation of Generations in Aphid development, as well as for other forms. He and Carus (1849) concluded that the viviparous development is comparable to the Cercaria stage of the Fluke worm, and the theory, first suggested by Duvau (1825), that here we have two generations, each distinct from the other, but each in turn giving rise to the other, was strengthened. Steenstrup would not, however, admit that the viviparous development is at all comparable to the oviparous, for he wrote: "No true ovary has been discovered in the larval and larviparous Aphids, but the germs, as soon as they are perceptible, are situated in organs which must be regarded as oviducts and uteri."[1]

About this time the theory of Dzierzon (1845) was advanced for the parthenogenetic development of the drone eggs of the Honey Bee, but such an explanation was not accepted for Aphids, and even v. Siebold, in his celebrated paper, "Wahre Parthenogenesis bei Schmetterlingen und Bienen" (1856), although advocating parthenogenesis for the forms on which he worked, refused to admit it for plant lice, for he wrote: "Die viviparen Blattläuse keine Weibchen sind, welche sine concubitu im jungfräulichen Zustande entwicklungsfähige Eier hervorbringen, sondern geschlechtlose mit Keimstöcken ausgestattete Ammen-oder larvenartige Individuen, welche von den wirklich jungfräulichen Blattläuse-Weibchen himmelivert vorschieden sind."[2]

Owen (1849) applied the term Parthenogenesis to the development of Aphids, not in the sense in which it is now used, but as an equivalent of the term Alternation of Generations used by Steenstrup. Owen thought that the fertilization which takes place in

[1] Page 112, English translation.
[2] Page 14, *loc. cit.*

the fall was enough to furnish what he designated as "spermatic force" for the development of the numerous summer generations. "In the vertebrated and higher invertebrated animals only a single individual is propagated from each impregnated ovum. Organized beings might be divided into those in which the ovum is uniparous and those in which it is multiparous. This is the first and widest or most general distinction which we have to consider in regard to generation, and in proportion as we may recognize its cause will be our insight into the true condition on which Parthenogenesis depends." [1]

The next step in advance was made when it was discovered by Leydig that there is no observable fundamental difference between the ova of the viviparous and oviparous females. There is, of course, a great difference between the summer eggs which develop parthenogenetically and the winter eggs as to size and amount of yolk, but this is only such a difference as may be observed between the eggs of various species and in no way argues for a dissimilar origin. This, then, put the Aphid development in the same class with that of *Solenobia*, *Apis* and other species known to develop from unfertilized eggs; but so firm a hold had the idea that fertilization is necessary to the development of a true egg that Huxley (1858) and Lubbock (1857) gave the name "Pseudova" to the eggs of the viviparous females. From this time on it has been held that the viviparous development was a case of true parthenogenesis.

The Alternation of Generations and parthenogenetic development is further complicated by other factors. Thus in Aphids the last of the viviparous generations is a generation known as the *sexupara*, the parthenogenetic and viviparous descendants of which are winged males and wingless females. After copulation, these females lay the fertilized winter eggs. This cycle of development is still further complicated by migrations from one plant host to another. A winged parthenogeretic generation frequently appears, and then may migrate to a different plant there to reproduce itself, and in a later generation return to the original host (Lichenstein, 1875). These generations have been distinguished by Blochmann (1889) as emigrants, alienocolæ and remigrants. Thus *Pemphigus terebinthi* (Derbes, 1872) gives rise to a wingless parthenogenetic generation (*a*), which produces another winged

[1] Page 62, *loc. cit.*

generation (*b*), the emigrants. This generation goes to another plant and produces a third generation (*c*), the remigrants and sexupara, which hibernate, return to the original plant and produce the small wingless sexual forms (*d*), the "sexuales." Here the sexual generation occurs in the spring rather than in the fall, as in most other forms.

Similar conditions are found in the Chermetidæ, except that here the parthenogenetic generations as well as the generations arising from fertilized eggs are oviparous (see the works of Blochmann, Dreyfuss and Cholodovsky). In *Chermes abietis* the fertilized egg develops into a wingless parthenogenetic female (*a*), which hibernates at the base of the buds of *Abies balsamia* and produces galls. In the spring winged females (*b*) are produced, which migrate to the Larch and give rise parthenogenetically to a wingless generation (*c*), which hibernates under the bark. These alienocolæ in the following spring produce parthenogenetic winged females (*d*), remigrants or sexupara, which return to *Abies* and produce wingless males and females, the eggs of which produce the first generation named in the cycle. Here two years is required to complete the cycle.

In *Phylloxera quercus* (Lichenstein) the winter eggs are laid on *Quercus coccifera* and give rise to females, which produce parthenogenetically a winged generation (emigrants), which fly to *Q. pedunculata* and *Q. pubescens*. These parthenogenetically produce several generations of alienocolæ and finally produce the remigrating sexupara, which return to *Q. coccifera* and produce the sexual generation. In *Phylloxera vastatrix* the generation which develops from fertilized eggs laid under the bark of the grapevine wander to the roots and there produce parthenogenetically several generations of wingless forms, which cause the swellings of the roots. This series is closed by the production of winged sexupara which go to the surface and swarm. Their eggs, which develop without fertilization, vary in size according to the sex, and the resulting individuals again begin the cycle.

The physiological difference between fertilized and parthenogenetic eggs is often accompanied by difference in appearance. The parthenogenetic ones are generally small and poor in yolk and develop in a shorter time and in greater number, while those requiring sexual cell union are larger and develop more slowly. The former are called summer eggs or *Subitaneier;* the latter winter

eggs or, because of the fact that they remain undeveloped for some time after fertilization, *Dauereier* or retarded eggs.

DIPTERA.—This group furnishes some excellent examples of pædogenesis or pædoparthenogenesis, and the phenomena as shown in various genera grade into each other in such a manner that it becomes evident that no line of demarcation can be drawn between parthenogenetic development from eggs laid by adult females and pædogenesis. While it is perhaps well to make a distinction between the phenomenon of parthenogenesis as exhibited by eggs of adult females and the same phenomenon as shown by the eggs of females which have not yet reached the last or adult stage of their development, yet the fundamental principle is the same in each case and it is not well to put too much stress on the degree of development of the parent when such a distinction tends to hide the similarity of the two kinds of reproduction.

Wagner (1862), in a Russian paper, reported cases of fly *larvæ* which bring forth young viviparously and, as he thought, from a transformation of the fat body, the parent dying at the birth of the offspring. This was in opposition to every principle of zoology and was, of course, not accepted on account of the announced method of formation of the embryos. In a short time, however, v. Baer (1863) and Meinert (1864) confirmed Wagner in all points except the source of the young, and later Wagner came to the conclusion that the viviparous young, are developed from true eggs. These conclusions were confirmed by Ganin (1865). The forms worked on were *Miastor* and *Cecidomyia*.

The next phenomenon of the series is that shown in *Chironomus* (Grimm, 1870). Here the *pupæ* lay eggs which develop parthenogenetically. This case comes nearer to what is observed in Hymenoptera, and the next step, which completes the series, is that of *Chironomus Grimmii* (Schneider, 1885) in which the *imago* lays parthenogenetic eggs.

Without going into a discussion of other forms on which work has been done, it will be evident that here we have a series of cases in which the Cecidomyidæ have reached the most specialized condition, they being able to bring forth young viviparously from a larval parent without waiting for the parent to reach the adult condition before acquiring sexual maturity. The case reported by Grimm for *Chironomus* would then appear to be one in which this power of bringing forth young very soon had not been so com-

pletely acquired, since the female must here wait until she reaches the pupal stage before she is sexually matured, and then she has not the power of viviparity but must lay her eggs; viviparous reproduction being undoubtedly an advantage to a species from the standpoint of increasing their numbers. It would then seem that some Diptera have not only acquired the advantage of parthenogenetic development but have shifted this power back to the pupa, or even larva, so that they may still more profit by this specialized method of reproduction.

ORTHOPTERA.—The development of eggs without fertilization has recently been described for this group by several persons. Dominique (1899) obtained parthenogenetic development (thelytoky) in *Bacillus gallicus*, while Heymons got one male to every twenty to twenty-five females in the parthenogenetic offspring of *B. Rossii*; and Azam (1898) and Stadelman (1898) also got some parthenogenetic individuals in the last-named species. Bolivar (1897 and '99) got three cases out of ten in which isolated larvæ of *Heptynia hespanica* produced eggs which developed; but he is not sure that they were not fertilized, although Pautel describes parthenogenesis as occurring in this species [cf. also Brunn (1898)].

From the evidence now at hand it would appear that parthenogenesis in this group is exceptional.

CRUSTACEA.

Next to plant lice, our earliest knowledge of the development of unfertilized eggs was for cases among the Crustacea. Schäffer (1755) described the development of eggs from unfertilized females of Daphnids, and by isolation he succeeded in producing several generations without fertilization and described this as being similar to what was known to take place in Aphids. Ramdohr (1805) raised ten successive generations parthenogenetically, and Jurine (1820) also confirmed the work. Ramdohr, however, did not look on these forms as true females but as hermaphrodites. These observations were on summer eggs, there being practically the same difference in this group as we find in Hemiptera. The summer eggs as in Aphids develop parthenogenetically, while the winter eggs require fertilization.

v. Siebold (1856) stated that he thought that *Apus cancriformis*, *Limnadia gigas* and *Polyphemus oculis*, in which no males had been

observed, showed true parthenogenesis, and Leuckart (1857) expressed the same opinion for *Daphnia*. In 1858 males of *Apus* were discovered and were examined by v. Siebold and he thus learned that some broods can go on developing parthenogenetically, like the Lepidoptera (Thelytoky), while other broods have both sexes present. For several years he watched a small pool near Munich, and at one time with great care removed every individual and found no males in 5796 individuals. In pools where both sexes occurred the proportion of males and females was very variable, and v. Siebold was led to believe that in these cases the males are disappearing, since from examinations in different years he found a constantly increasing proportion of females.

v. Siebold foresaw the objection that males might have been present previous to the examination of the pools, and consequently examined the male genital organs and spermatozoa and then the ovaries and their development. He never succeeded in finding any spermatozoa in the female genital organs. The structure of the ovum made this observation decisive since he found a hard egg-shell formed in the uterus and no micropyle, so that if fertilization takes place it must be before the egg is laid. Brauer (1872) found that fertilized eggs of *Apus* produced males.

Several other groups of Crustacea show a similar method of development, but do not differ to any extent from *Apus*. Parthenogenesis has been observed in the Phyllopods, Ostracods and Copepods, but in none of the Malacostraca.

In *Artemia salina*, Joly (1840) found no males in 3000 individuals examined and explained this as due to hermaphroditism, but Gerstäcker (1867) and especially v. Siebold (1871) established this as a case of true parthenogenesis. In *A. Milhausenii*, Fischer v. Waldheim (1834), Rathke (1836) and Fischer (in Middendorf's *Reise, Zoologie*) found that males are rare, and the same is true for *Limnadia Hermanii*, both cases being explained like that of *A. salina*. The maturation of the parthenogenetic egg of *A. salina* (Brauer, 1893) is discussed in another place.

A case worthy of note is that of *Leptodora hyalina*, a Daphnid, in which the winter eggs follow the usual plan of Crustacean development and form a Nauplius stage, while the summer eggs develop directly into an adult form with all limbs present. This is one of the striking cases which indicate that parthenogenesis is acquired

where it is desirable to produce individuals quickly, since here the larval stages are omitted.

Unisexual and bisexual generations alternate with each other in various ways in Crustacea and the mode of the alternation is remarkably related to their environment, as has been shown by Weismann. According to whether the causes of destruction visit a colony once or several times during the year we find forms which have one or several cycles of parthenogenetic and bisexual generations, and finally species are known which show no alternation. These are designated as monocyclical, polycyclical and acyclical respectively.

Trematodes.

The development of the *cercaria* and *redia* stages of *Distomum* has been the subject of much discussion for a long time. Leuckart, in his *Parasiten des Menschen*,[1] gives an historical account of our knowledge of the development of these forms up to the date of its issue (1879). That there is a development without fertilization is admitted on all sides, but the question as to whether the *rediæ* develop from true germ cells is still a point of dispute. Leuckart (1882) and Schwarz (1886) consider this as a true case of pædogenesis, the internally developing *rediæ* being looked on as arising viviparously from cells of the germinal epithelium. On the other hand, Wagener (1857) and Biehringer (1885) maintain that they arise from cells of the body wall and are therefore not produced sexually but by budding. Korschelt and Heider, in their *Text-Book of Embryology* (1890), do not consider this difference of great significance. "[2]This difference does not seem to us to be important, for we have already seen that the parietal cells and the germ cells are embryologically of the same origin. In a portion of the cells of the body wall even, a differentiation into separate histological elements appears not to have taken place, and for this reason they may continue to develop in the same way as the real germ cells. In harmony with this view is the statement of Thomas,[3] who derives the rediæ from both the germ cells and the cells of the body wall; if the supply of the former were exhausted, then the latter might take their place."

[1] II. Bd., p. 488 and following pages.
[2] Page 183, Vol. I, English Translation.
[3] Thomas (1883).

If such an explanation be the true one, then it would appear that the difference between sexual and asexual reproduction is not so great as is generally supposed.

Rotifers.

The phenomena of development are very complicated in the Rotifers. In most cases the males differ from the females in being smaller and in the absence of an alimentary canal. The eggs are of two kinds, the same difference being seen here between summer and winter eggs as in Aphids. Cohn (1856–58) first worked out the development of this group and found that the winter eggs are fertilized, while the summer eggs are not. Huxley (1857) looked on these summer eggs as sexless buds, but the work of Joliet (1883), Plate (1884–85) and Maupas (1889–90) established this as true parthenogenesis. Here, as in Aphids and Daphnids, the males appear at the beginning of an unfavorable period in the life cycle.

Under the subject of the Maturation of Parthenogenetic Eggs the work on Rotifers is mentioned, and the results there recorded are the most interesting features in counection with the phenomenon of parthenogenesis in the group. The principal point of interest is that the male and female eggs behave differently during their maturation, although eggs of both sexes have the power of development without fertilization.

Lauterborn (1898) found that the Rotifers could be classified into three groups as follows: (1) Species found all the year around; (2) Species found in summer, and (3) Species found in winter. In the summer and winter species the fertilized and yolk-laden eggs appear after a long series of parthenogenetic generations; they are monocyclic. In the species found during all seasons of the year the appearance of the males and the consequent fertilized eggs may occur twice or more times during the year; they are polycyclic. Probably some species are acyclic; that is parthenogenetic forms can be produced indefinitely and "winter" eggs are unknown. The determination of the appearance of males in Rotifers has been variously explained, the amounts of heat (Maupas) and nutrition (Nussbaum) being often considered as the causes. Lauterborn concludes that such external causes do not fully explain this but that some internal factor is the principal cause. The cyclic appearance

of fertilized eggs recalls the periodic occurrence of conjugation among protozoa, and according to Wesenberg-Lund (1898) and Lauterborn (1898) senility is an important factor in determining the length of the cycle. That lack of nutrition and the appearance of a senile condition are intimately connected seems very probable, if we may be permitted to reason from analogy on work done on *Paramœcium caudatum*. Calkins, in a recent paper,[1] records that he has been able to raise Paramœcia for six hundred and sixty-five generations by fission, and they were rejuvenated five successive times by change of food rather than by conjugation, or as he expresses it "parthenogenetically."

Vertebrates.

The question as to whether there is a parthenogenetic development among any of the Vertebrates is one which has been much discussed. If there are any cases at all they are cases of partial parthenogenesis, since in no case is it claimed that development goes farther than the first few cleavage stages. Bonnet (1899) discusses at some length the evidence on this subject, and since he has so well reviewed the literature it is not necessary to do more here than state the general conclusions to be reached from a survey of what has been reported.

Eggs of *Amphioxus lanceolatus* (van der Stricht, 1895) show a tendency to divide if not fertilized. This is not pronounced.

Cleavage of unfertilized eggs in the ovary are reported among the Gadidæ by Burnett and Agassiz (cited by Oellacher, 1869), for the Sturgeon by Bellonci (1885), and for the Trout by Oellacher (1872). Oellacher attributed this to the retention of the eggs for too long a time.

For the Frog and other Amphibia many investigators have claimed parthenogenetic cleavage, since it frequently happens that eggs which pass from the female when she is not copulating with a male show cleavages, but these are generally irregular. Pflüger (1882) was able to show rather conclusively that such cases are due to fertilization of these eggs by spermatozoa in the water which are nearly dead, and consequently the development is short and irregular. Kulagur (1895) and Bataillon (1900) did some experiments

[1] Calkins, Gary N., 1902, "Studies on the Life History of Protozoa," III. The Six Hundred and Twentieth Generation of Paramœcium caudatum, *Biol. Bull.*, III, No. 5, pp. 192–205.

on artificial parthenogenesis, but these must not be considered as arguing for a true natural parthenogenesis.

Among Reptiles, Strahl (1892) reports irregular parthenogenetic cleavages.

In the Birds, the evidence for a parthenogenetic development is perhaps the strongest of that for any vertebrates. It frequently happens that a blastoderm is formed on an egg which is apparently not fertilized. Cases of this kind are found in the Chick (Coste, 1859; Oellacher, 1869; Koelliker, 1879, and others), in the Turtle Dove (Motta Maja, 1877; cited by Duval, 1884), and for several other birds (Duval, 1884). Balfour (1880) pointed out that care must be exercised in passing judgment on these cases, since it is known that spermatozoa can live for a considerable time in the female and that possibly these are really cases of fertilized eggs. This is entirely upheld by the later work of Lau (1895) and Barfurth (1895), who show that eggs from virgin hens do not show cleavage in the same way as do those from hens which have copulated with a cock even a considerable time before. In eggs from virgin hens the blastomeres do not have a cellular character, since all but a few lack nuclei, and when nuclei are present they do not divide mitotically. The blastoderm lacks all power of assimilation, the blastomeres are irregular and the whole shows no thickening at the posterior end. There is never a segmentation cavity. Lau and Barfurth looked on such cases as due to a physico-chemical process, caused partly by evaporation and partly by coagulation of the protoplasm. Cases in which the female has previously copulated would then appear to be similar to those of the frog, in which there is a fertilization accomplished by a partially devitalized male cell.

Even in Mammals, cases are recorded of the cleavage of the egg while still in the Graffian follicle. Janosik (1896) reports several cases (Rabbit) in which a cleavage has taken place, a semblance of a cleavage cavity formed and the whole mass has broken away from the membrana pellucida as in normal development; but the phenomenon is so evidently connected with disintegration from the very beginning that it must not be considered as parthenogenesis.

The question as to whether Dermoid cysts are due to the parthenogenetic development of an egg has received a great deal of attention, and the exceptional case reported by Répin (cited by Duval, 1895) would point strongly to such an explanation as the true one. This cyst had four limbs and terminated in a kind of

head composed of bones arranged in a cube and surmounted by three teeth. The bones of the feet and hands were perfectly recognizable. There was no alimentary canal in the body, but beside it was a tube which histologically resembled an intestine.

It would appear then that these phenomena are not true parthenogenesis, unless it be that we consider that as the explanation of the cysts. The fact that the cleavages do not follow the regular plan of fertilized eggs would not of itself bar these cases as being classed as a true development from an unfertilized egg, since in other cases, where there is undoubtedly a parthenogenetic development, the method of growth differs from that of the fertilized egg of the same species, *e.g.*, *Leptodora*. Neither must we bar these cases because the development goes but a short distance, since the life of an individual must be considered as beginning with the unsegmented egg, and if that egg shows a power of development without fertilization, that phenomenon is as truly parthenogenesis as if an adult animal resulted. However, since in these cases we find the segmentation of the egg to be more in the nature of a physico-chemical change than a true cleavage, we must consider it as entirely different, and we must, of course, bar out all cases in which the proper amount of care has not been taken in proving that fertilization has not been affected by a half-dead spermatozoon.

Arachnids.

But one well authenticated case is known to exist in Spiders (Campbell, 1883). Parthenogenesis in this group has recently been discussed by Montgomery (1903), and it is not necessary to repeat his discussion since it has been done so recently.

In many other animals there is a marked tendency for the mature egg to go on dividing if fertilization does not take place. This is often observed in Echinoderms, some Annelids and Molluscs. Such eggs never develop beyond a very early stage, and only a very small proportion of eggs show this cleavage. A point worthy of note is that these very forms are the ones which have yielded the best results in work done in Artificial Parthenogenesis, and the explanation which seems to follow from this is that such eggs normally require a very small amount of stimulus from the male cell, and the addition of some chemical to the water is enough to take the place of the male stimulus. In fact the results of artificial

parthenogenesis differ from what is normally found only in the greater proportion of parthenogenetic eggs.

The Maturation of Parthenogenetic Eggs.

The main point of interest in parthenogenesis is perhaps that of the maturation of the parthenogenetic eggs, on account of its general bearing on the theory of fertilization and on account of its support of the theory of the individuality of chromosomes

Minot (1877), in an article on the theoretical meaning of maturation, suggests that parthenogenesis may be due to failure to form polar bodies, and since the entire mass of chromatin remained in the egg it would be hermaphrodite and capable of development without the addition of any chromatin from the male cell. Balfour (1880) follows out the same line of thought in suggesting that the function of forming polar bodies has been acquired by most ova to prevent parthenogenesis, and van Beneden (1883) held a nearly similar view.

Weismann (1886) found that one polar body is given off in the case of *Polyphemus* (Daphnid), and he later determined the same thing for parthenogenetic Ostracodes and Rotifers.[1] Blochmann (1888) found in Aphids that one polar body is given off in the case of eggs which develop parthenogenetically, while two are produced in eggs which require fertilization. Weismann was thus led to the view that the second polar body is of special significance in parthenogenesis. In insects (Blochmann and others) the polar bodies are not thrown out of the egg as in most other animals, but the chromatin masses remain embedded in a vesicle in the protoplasm of the egg, near the periphery, and are called "polar nuclei."

Boveri (1887) found in *Ascaris megalocephala* that the second polar body might remain in the egg (as is normally the case in insects) and give rise to a nucleus indistinguishable from the pronuclei. He, therefore, suggested that parthenogenesis might be due to the retention of the second polar body in the egg and its use as a male pro-nucleus.[2] "The second polar body would thus, in a certain sense, assume the rôle of the spermatozoon, and it might

[1] Compare Lenssen (1899), Erlanger u. Lauterborn (1897) and Mrazek (1897).

[2] Boveri (1887), p. 73.

not without reason be said: Parthenogenesis is the result of fertilization by the second polar body."

This conclusion was in part confirmed by Brauer (1893) on the parthenogenetic egg of *Artemia salina*. There are two types of maturation in parthenogenetic eggs occurring in the same animal, one in accordance with the idea of Boveri and the other not irreconcilable with it. For a brief description of the two methods in *Artemia* the statement of Wilson[1] is quoted:

"In both modes typical tetrads are formed in the germ-nucleus to the number of eighty-four. In the first and more frequent case but one polar body is formed, which removes eighty-four dyads, leaving eighty-four in the egg. There may be an abortive attempt to form a second polar spindle but no division results, and the eighty-four dyads give rise to a reticular cleavage-nucleus. From this arise eight-four thread-like chromosomes and *the same number appears in later cleavage stages.*

"It is the second and rare mode that realizes Boveri's conception. Both polar bodies are formed, the first removing eighty-four dyads and leaving the same number in the egg. In the formation of the second, the eighty-four dyads are halved to form two daughter groups, each containing eighty-four single chromosomes. *Both these groups remain in the egg and each gives rise to a single reticular nucleus, as described by Boveri in Ascaris. These two nuclei place themselves side by side in the cleavage figure, and give rise each to eighty-four chromosomes, precisely like two germ-nuclei in ordinary fertilization.* The one hundred and sixty-eight chromosomes split lengthwise and are distributed in the usual manner, *and reappear in the same number in later stages.* In other words, the second polar body here plays the part of a sperm-nucleus precisely as maintained by Boveri.

"In all individuals arising from eggs of the first type, therefore, the somatic number of chromosomes is eighty-four; in all those arising from eggs of the second type, it is one hundred and sixty-eight. This difference is clearly due to the fact that in the latter case the chromosomes are single and univalent, while in the former they are bivalent (actually arising from dyads or double chromosomes). The remarkable feature, on which too much emphasis cannot be laid, is that the numerical difference should

[1] Wilson, *The Cell in Development and Inheritance*, pp. 281–284.

persist despite the fact that the mass and, as far as we can see, the quality of the chromatin is the same in both cases."

Blochmann (1889) studied the maturation of drone and worker eggs in *Apis mellifica* with the following results: The first polar nucleus is given off normally and remains undivided, but the second polar nucleus often appears to divide. The fact that these three nuclei are not, as in some cases, due to a division of the first polar nucleus is proven by the position of this nucleus, which is always found just under the surface of the egg and separated by some distance from the other two. The female pro-nucleus soon becomes vesicular in form and goes to the axis of the egg, where it forms a spindle and gives rise to the blastoderm cells. The polar nuclei change as in *Musca vomitoria*, but do not become vesicular in form, approach one another and are enclosed by a rather large vacuole of the superficial protoplasm, which is free from yolk. In this vacuole they break up into fine chromatin granules, which become scattered through the whole cavity of the vacuole. We may suppose that the contents are later removed from the egg. In fertilized eggs the ovarian nucleus undergoes the same divisions as the unfertilized.

Platner (1887) also found two polar nuclei in *Liparis dispar*, a parthenogenetic Lepidoptera. These two cases, the first two recorded, are not in accord with the previous views of Weismann, and in 1891 he sought to explain these cases as follows: "Das Kernplasma einzelner Eier einer Art das Vermögen des Wachsthums in grösserern Masse als du Majorität derselben besitze, oder, im Falle der Biene, jedes Ei besitze de Fähigeit, sein auf die Hälfte reducirtes Kernplasma, wenn es nicht durch Befruchtung wieder auf das Normalmass gebracht wird, durch Wachsthum wieder auf die doppelte Masse zu bringen."

Petrunkewitsch (1901), studying *Apis*, found that eggs laid by the queen in drone cells never showed any signs of having been fertilized. As in a fertilized ovum the first polar nucleus is separated by an equatorial division, in the second maturation there is a reduction of chromosomes to one-half. Similarly the first polar nucleus always divides with a reduction and the peripheral half is liberated and perishes. The restoration of the number of chromosomes in non-fertilized eggs probably occurs by a longitudinal splitting of the chromosomes, but with a suppression of the corresponding division into two daughter nuclei. The central half of

the first polar nucleus conjugates regularly with the second polar nucleus and forms a "Richtungscopulationkern," with the normal number of chromosomes. This nucleus in the drone egg gives rise by three divisions to eight cells with double nuclei. In fertilized ova and in drone eggs laid by fertile workers this nucleus forms a spindle, which either simply disappears or gives rise to a number of nuclei, one to four; but these always show disruption phenomena in the chromosomes and ultimately disappear. In a later paper the same author (Petrunkewitsch, 1902) asserts that the products of the Richtungscopulationkern ultimately become the testes of the adult drone.

Paulcke (1899) found that in drone eggs there are four groups of chromosomes. Of these two seem to be the result of division of the first polar nucleus, one of the second polar nucleus and the fourth the egg nucleus. In twelve eggs examined from worker cells, fifteen minutes after they were laid, eight show sperm nuclei with their radiating systems. In eight hundred drone eggs examined no sperm nuclei were seen, but in three cases dark corpuscles were observed, which might have been sperm nuclei. In fertile worker eggs there were no indications of male pro-nuclei.

Mrazek (1897) and Erlanger und Lauterborn (1897) studied the maturation of the eggs of *Asplanchna*, a Rotifer. They find in this genus three kinds of eggs: (1) Parthenogenetic male eggs; (2) parthenogenetic female eggs, and (3) female eggs which require fertilization. When the female eggs requiring fertilization begin to develop, all other eggs begin to show cleavages of a degenerative nature, not like the normal cleavage, probably due to lack of nutrition (Mrazek). The parthenogenetic female eggs give off *one* polar body which never divides, while the parthenogenetic male eggs give off *two* polar bodies, the first of which normally divides. The female eggs requiring fertilization act like the parthenogenetic male eggs. In the parthenogenetic male eggs there is no indication of a union of the second polar body with the egg. The number of chromosomes is not determined (Erlanger und Lauterborn).[1]

Rückert (1895) found that in *Cyclops sternuus* the second maturation division cuts off a polar nucleus which remains in the egg, in a direction tangential to the second division figure. It does not

[1] See also Lenssen, 1899, "Contribution à l'Etude du developpement et de la maturation des œufs chez *l'Hydatina sexta*," *Cellule*, xiv, pp. 421–51, 2 pl.

form the primordial germinal cells. The first maturation division gives off a true polar body.

Causes of Parthenogenesis.—When we consider the difference in behavior of various parthenogenetic eggs during maturation and the differences in sex relations exhibited by the various groups, together with the wide range of the scattered cases where such development occurs, it is evident that parthenogenesis has had a separate origin in many places in the animal scale. All that is necessary in the maturation of a parthenogenetic egg is that the normal number of chromosomes shall be retained, and this may be brought about by the retention of the second polar body, fertilization by the second polar body or perhaps by the division of the chromosomes without the corresponding cell division.

In seeking for a cause for the appearance of parthenogenesis in a group of animals, it must be borne in mind that we are dealing with a phenomenon that to all practical purposes is like asexual reproduction, in that the species is not dependent on the union of the two sexes for the propagation of all the individuals of the species and that the causes for the appearance of asexual and parthenogenetic reproduction are practically identical, it being merely a question as to which method of agamic reproduction is most readily acquired by a given form when the necessity for such a thing arises. And, too, it is probable that the cause is not the same in all cases, since the environments and habits of the various forms possessing this power are so varied.

In the first place, parthenogenesis is generally associated with and probably caused by the necessity of the appearance of a great many individuals suddenly at a certain period of the year or of the life cycle. A large part of the forms exhibiting this method of reproduction are small short-lived animals which are represented during the winter or some adverse time in the life cycle by a very few individuals and, in order that the species may survive, are compelled to acquire some method of rapid agamic reproduction.

In the case of the Aphids the necessity is for females and we find thelytoky evolved; in the case of the Honey Bee the necessity is for males, so that the queens may not go unfertilized, and we find arrenotoky.

The question of economy enters very largely into the problem and is, in fact, almost identical with the preceding cause. In many

cases males are exceedingly rare at all times or except at certain seasons, and it is manifestly to the advantage of the species if it is able to survive without the presence of any but propagating individuals. Thus in the case of the bee, previously mentioned, it would be detrimental to the species to have countless drones feeding on the hive supplies during the winter; but for the purpose of increasing the hereditary influence, it is beneficial to the race to feed these males for a brief period when food is plentiful, in order that the fertilization may bring about the results known to come in all cases from such a union.

In still other cases the very habits of the animal make the chance of the occurrence of a sexual union too small, and in consequence the females have acquired the agamic methods of reproduction. The case of *Cercaria* offers a good example of this. If we accept the conclusions of Thomas, we see that here we get a transition from unisexual to asexual reproduction; and while these two processes are usually widely separated, yet the same difficulty of a sexual union may be looked upon as the probable cause of either phenomenon.

Determination of Sex.—From what has gone before we see that the problem of sex determination is very closely related to that of parthenogenesis, since parthenogenetic eggs so frequently show such peculiar sex relations. In some groups unfertilized eggs produce only males (arrenotoky), in others only females (thelytoky), while in some both sexes are produced (amphoterotoky). Taking as an example the Honey Bee, we know that the male eggs are not fertilized and the female eggs are; and reasoning from this, it seems true that the act of fertilization is the one determining factor, since no one has yet been able to find any other fundamental point of difference. As was shown under another heading, other explanations, such as differences in food or size of cell, have been advanced, but these have already been answered. Such work as that of Mrs. Treat (1873) on Caterpillars, of Born (1881) and Yung (1881) on Amphibia, and of Nussbaum (1897) on Rotifers would seem to indicate that lack of nourishment favors the production of males; but until we have more evidence we are perfectly justified in explaining these cases as simply survivals of the more fit sex under trying conditions, and cannot use them as arguing for theories like those of Dickel. In fact Cuénot (1899) did not succeed in verifying the results of Mrs. Treat, for he found that the proportions of

males and females remained approximately the same under all food conditions, and concluded that sex is determined in the ovary in insects.

There have recently appeared two papers of interest in this connection as offering suggestions for future work. Beard (1902) and v. Lenhossek (1903) conclude, on theoretical grounds, that sex is determined in the ovary of the mother and that there are in all cases two kinds of eggs, male and female, fundamentally differing from one another. Cases where such a state of affairs is known to exist are *Phylloxera*, *Dinophilus*, some Rotifers and possibly in *Raja batis* (Beard, 1902). According to these views, the sex is determined before leaving the ovary and consequently fertilization can have no influence, but at present we cannot look on these theories as more than interesting suggestions. It must be admitted that the determination of sex by fertilization is in direct opposition to what we know to be true for the great majority of animals where both sexes alike arise from fertilized eggs, and on *a priori* grounds the theory of Cuénot, Beard and von Lenhossek seems probable; but in this instance, as in all others in zoology, *a priori* reasoning is unsafe and we must wait for future investigations to decide whether there is any truth in these suggestions.

Comparison of Various Sex Relations.—As has been pointed out by several investigators, the process of fertilization has two distinct purposes—the giving of a stimulus for development to the mature egg, and the increasing of the number of hereditary tendencies of the offspring by giving it a blending of hereditary traits from two parents. The power of parthenogenetic development possessed by some animals takes the place of the stimulation of the male sex cell, since the ovum has given to it in the ovary enough vital force to go on dividing mitotically even after it becomes a part of another generation.

The second office of fertilization is simply omitted where fertilization does not occur, the advantage of agamic development more than balancing the advantage to be gained by the meeting of two lines of heredity. During ordinary maturation the egg gives off in its polar bodies one-half of the number of its chromosomes, the heredity carriers, and by the acquisition of an equal number from the male cell, carrying hereditary tendencies from the male parent, the original number is regained; and in order that the normal num-

ber may be retained in parthenogenetic eggs the reduction division is omitted, or in some other way the same result is accomplished.

This omission of a mixing of two lines of ancestry in the reproduction of a species is, if our conception of its significance is correct, a very important one. There is, however, a great difference in the extent of this omission in the various kinds of parthenogenesis. In Arrenotoky at every second generation a crossing occurs *of necessity*, since the females are produced from fertilized eggs. In Thelytoky, on the other hand, a mixing may be very rare or even entirely wanting; while in Amphoterotoky it generally occurs at regular intervals, as in the fall in Aphids. On the other hand, Thelytoky and Amphoterotoky are much more beneficial to a species from the standpoint of its propagation, since at no time is fertilization an absolute necessity, while in Arrenotoky fertilization is necessary for the production of the individuals which do the most toward the reproduction of the species. What the species loses in hereditary influences is more than made up by the increased advantage of these two most specialized kinds of parthenogenesis.

Pædogenesis.—If we look on parthenogenesis as a phenomenon which has arisen in various groups of animals so that the species may be reproduced rapidly and without so much dependence on chance, then it is but another step in the same direction to find this process shifted back to an embryonic stage of development so that the reproduction would not be delayed until the female reached the adult state. The same precocious segregation of the reproductive process is met with in forms which always require the fertilization of the egg, *e.g.*, *Amblystoma* (Axolotl), but in these cases the coincident phenomenon of parthenogentic development has not been necessary or desirable and we distinguish such cases as Proiogony. We may look on certain groups of the Diptera as in a transition stage, between the parthenogenesis like that observed in *Chironomus Grimmii* and that of *Miastor*. The species of *Miastor* has still further acquired the advantage of viviparity for the protection of the youngest embryonic stages, and seems almost to have reached the limit of advantage that a species can acquire for the propagation of its kind.

Partial Parthenogenesis.—As has been seen, eggs which have not been fertilized often begin to develop, but after a short time die. On this account it has been argued that such cases are not really parthenogenesis, since an adult or a sexually mature individual does not

result from the division. Such an argument cannot hold, since the fundamental principle involved is the same whether an adult results or not. We must consider that the life of the individual begins with the unsegmented egg, and if that egg has in itself the power of growth, manifested by cell division, then we must class it as a parthenogenetic egg; the only difference between such cases and examples like the male eggs of the bee being that there is in the former not so much of the power of unisexual development: it is merely a difference in degree and not in kind. It would seem that many of the cases of artificial parthenogenesis described are exactly similar to these cases of partial parthenogenesis, and that the change in environment produced artificially simply allows the egg the power of growth already in it to go on for a short time exactly as if fertilized.

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Stated Meeting, November 6, 1903.

President SMITH in the Chair.

A letter was read from the Schlesische Gesellschaft für Vaterländische Cultur, announcing the celebration of its one hundredth anniversary on December 17, and inviting the Society to send a representative to take part in the celebration. The Hon. Charlemagne Tower was thereupon appointed as such representative.

The decease was announced of Prof. Robert Henry Thurston, at Ithaca, on October 25, æt. 64.

Prof. Charles F. Chandler, of New York, read a paper on "The Electro-Chemical Industries at Niagara Falls."

Dr. Hans Goldschmidt, of Essen, Germany, explained his method for producing intense heat by his Thermite process.

A paper on "Dying American Speech-Echoes from Con-

necticut," by Prof. J. Dyneley Prince and Frank G. Speck, was read.

The Amendments to the Laws recommended by the Officers and Council, and duly proposed at the meeting of May 1, were adopted.

DYING AMERICAN SPEECH-ECHOES FROM CONNECTICUT.

BY J. DYNELEY PRINCE, PH.D., AND FRANK G. SPECK.

(*Read November 6, 1903.*)

It was my good fortune last summer to light upon a small and little-known reservation on the west bank of the Housatonic river, about two miles south of Kent, Litchfield County, Conn., occupied by sixteen Skaghticoke Indians. There are, however, about one hundred and twenty-five individuals not on the Reserve who claim tribal rights and relationship with this clan. The present Indians on the Reservation are mixed with a very appreciable percentage of negro and white blood and, according to their own account, came originally from various Connecticut tribes. The clan is said to have been founded in 1728 by one Gideon Mawehu (the modern family name Mawee, evidently a corruption of English Mayhew) who was either a Pequot or a Wampanoag. The ranks of the Skaghticoke settlement were swelled by refugees and stragglers from other tribes, until in 1731 they reckoned one hundred and fifty warriors. DeForest mentions among these foreign elements Potatucks from Newtown and Woodbury, Paugussets from the upper Housatonic territory, Salisbury and Sharon Indians originally from Windsor, besides Pequots, Narragansetts and Wampanoags. This mixture of race is evidenced in the various loan-words of New England origin pointed out below by Professor Prince.

From one man, James Harris, who claims to be a full-blood and whose skin certainly shows the dark red hue characteristic of the eastern Algic races, I was able to obtain in the old language twenty-three words and three connected sentences which Professor

Prince has analyzed below. Harris has only a vague and disconnected idea of the language. What little he knows he learned in early youth from his grandmother, one of the Mawee family, who, according to his statement, had a connected speaking knowledge of the ancient idiom. The present Skaghticokes are Indians more by tradition than fact, and with the single exception of Harris have little of interest to impart to Americanists.

The name Skaghticoke was originally pronounced *p'ska'tikuk*, *i. e.*, "at the forked river," from the same stem as Abenaki *p'skaôî'kwen* "branch"+the ending *-tukw*, which always means "river" in composition. The river-names Piscataquis (Maine) and Piscataqua (New Hampshire) are undoubtedly corruptions of the same word and have an identical meaning (see Prince, *American Folklore Journal*, 1900, pp. 125 ff.).

FRANK G. SPECK.

Thanks to the efforts of Mr. Speck, who is a student in my department in Columbia University, a modern form of the ancient Pequot-Mohegan dialect has been discovered in its last throes (see Prince and Speck, *American Anthropologist*, V, pp. 193–212). Mr. Speck has now found the still more scanty remains of another Connecticut language, that of the Skaghticokes, which, as will appear from the following exposition, is probably the last surviving remnant of the Delaware-Mohican idiom formerly used at Stockbridge, Mass., which was expounded by J. Edwards, Jr., and J. Sergeant (see Pilling, *Bibliography of the Algonquian Languages*, *s. v.* these authors). This Skaghticoke language is distinctly not a New England product, but came from the Hudson river region with that branch of the Lenni Lenâpe called Mohicans who settled at quite an early date on the site of Stockbridge, Mass. This *Mohican* idiom is only indirectly connected with the *Mohegan*[1]-Pequot language just mentioned, found by Speck at Mohegan, near Norwich, Conn. Perhaps the longest specimen of the Stockbridge Mohican tongue has been preserved in J. Quinney's *Assembly Catechism*, printed at Stockbridge in 1795. For the modern dialect of the Delaware Lenâpe, see Prince, *American Journal of Philology*, XXI, pp. 295–302.

[1] Note that *Mohican* and *Mohegan*, although both forms of the same word, are now used purely arbitrarily, the first to indicate the Hudson River Lenâpian Mohican clan, and the second to denote the Pequot mixed race at Mohegan, near Norwich, Conn.

The name Mohican=*Mŭhĭgănĭtŭk* means "those dwelling on the tide-water" from Del. *makhaak* "great" and *hican* "tide" (so Zeisberger) and plainly shows the geographical origin of the tribe. How this name came to be applied to the Pequot-speaking Mohegans of Mohegan, Conn., has been explained at length by us (*Anthropologist*, V, pp. 194 ff.). The Skaghticokes apparently do not know the name Mohican as applied to themselves.

It is curious and characteristic of human nature that a number of obscene words and phrases have survived with some accuracy in the mouth of Harris, Mr. Speck's informant. Such words would naturally live longer than others in the speech of the uncultivated, no doubt owing to their desire to speak of such subjects with secrecy.

It is quite plain that Harris has only a very imperfect knowledge of his grandmother's language, as he does not know the exact meaning of two out of the three sentences which he gave to Mr. Speck. His three connected sentences are as follows:—(1) *Wĭchōwân tăpă-sŭk sûkăgĭtĭnŏn*, "hurry up to the hotel and get a drink." This seems to me to mean "come along, my friends, and we will have a drink." See *Glossary*, *s. v. kăgĭtĭnŏn*, *tăpăsŭk* and *wĭchōwân*. (2) *Gŭkwĭ dĭă n'pûmĭăs* "go sleep in the barn." This should be translated "you sleep there for the night." See *Glossary*, *s. v. gŭkwĭ* and *n'pûmĭăs*. (3) *Nŭnŭ'pă mănŭk* "lift up your clothes," said with obscene intention to a woman. This translation is correct, as will appear *s. v. nunu'pa* and *mănuk*. Finally, in this connection, it should be noted that Harris gives the incorrect forms *mămĭtŭkkŭ* for *mănĭtŭkkŭ* "devil"; *nĭskâhĭkĭăn* for *mĭskâhĭkĭăn* "cider," *n'pûmĭâs* for *n'pûwĭâs* "at night" and *tăpăsŭk* for *nĭtăpĕsŭk* "my friends" (see *Glossary*, *s. v.* these words).

The following words of the vocabulary are all Delaware Mohican: *gŭkwĭ* "thou sleepest"; *kwŏn* "yes"; *mămĭtŭkkŭ* for *mănĭtŭkkŭ* "devil"; *nĭskâhĭkĭăn* for *mĭskâhĭkĭăn* "cider"; *n'pûmĭâs* for *n'pûwĭâs* "at night"; *šâmût* "tripe"; *škûk* in *škûkărĭš* "snake"; *tăpăsŭk* for *nĭtăpĕsŭk* "my friends"; *tŭlĭpâs* "tortoise"; *wĭchōwân* "come along." On the other hand, the following are probably New England loanwords from native Connecticut dialects akin to the Natick:—*chăkŭs* "negro"; *kănŭkwŏk*, pl. of *kĭnkăi* "private parts"; *mănŭk* "coat," "petticoat"; *rŭtĭg* "crushed corn"; *skwă* "woman"; *sŭkkŭtăš* "succotash"; *tĭpĭ* "devil"; and *wănŭx* "white man." These loanwords are, of course, not surprising in

a language spoken in such an environment. The words *kwŏn* (=Del. *gohan*, but Natick *ôô*; Peq. *nux*=yes, so Stiles[2] in his vocabulary) and *špûti* "anus"=Del. *saputti*, would alone be sufficient proof of the Lenâpian character of the Skaghticoke idiom.

The Skaghticoke actually preserves the *r*-sound, so rare in modern Algic, in the words *rûtĭg* "crushed corn" and *škûkăriš* "snake." This is, so far as I am aware, the only modern instance of *r* in Algic, except in one dialect of the northern Cree. The *r* undoubtedly existed in Lenâpe at the time of the Old Swedish occupation of New Jersey and Pennsylvania (see Brinton, *The Lenâpe and their Legends*, p. 96 and, below, *s. v. rûtĭg*). As was the case among the Abenakis, this *r* changed to *l* at a very early date. In Rasles' dictionary of the ancient Abenaki, it is the regular rule to find *r* for modern *l*, but no living Abenaki pronounces *r* in the modern language. A most interesting parallel case is found in the Iroquois idiom spoken at the St. Regis Falls Reservation, where the Indians, instead of the *r* so common in Iroquois speech, now pronounce a thick medial consonant between *r* and *l*. Only the old people retain the primitive *r*-sound. My Iroquois informant tells me that a pure *l* will probably be pronounced by the next generation.

I must regard it as most fortunate for students of Algic philology that Mr. Speck has been able to collect these scanty and incorrectly preserved relics of a lost Algonquian language.

Glossary of Skaghticoke Words.

Chákŭs "negro" is undoubtedly cognitive with Stiles's Pequot *auchugyeze* "blackbird" which must stand for *chokêsu*; cf. RW.[3] *suckésu* "he is black" from *sucki* "black." The Del. *sukachqualles* "negro" is evidently a more distant cognate. I believe that *chákŭs* was a New England loanword among these Skaghticoke Indians. The Aben. *mkazawigit* "negro" is perhaps cognitive with Natick *mûi*, the regular word for "black" in that language.[4]

[2] President Stiles was the author of a Pequot vocabulary, the MS. of which is now in Yale University Library. This glossary is extensively quoted in J. Trumbûll's *Natick Dictionary*, Washington, 1903.

[3] RW denotes Roger Williams in his *Key into the Language of America*, which is a treatise on the Narragansett idiom.

[4] In indicating the pronunciation of the Skaghticoke words in this article, I have used the Italian vowel values, except *ú*=*u* in "but," and '=a short inde-

Gŭkwí dîâ "you sleep there," from *k*=2 p.+*kawi* "sleep" (=Del. *gauwin*, Aben. *kawi*); *dîâ*=*talli* "there." Cf. Peq. *dâi*=*dali*, Aben. *tali*; *dali* after a vowel.

Kâgtlínŏn "get a drink" (so Harris); I think the full form is *k'sûkâgitlínŏn* "we (incl.) shall drink," see below *s. v. wíchŏwân*. *Kâgî* is the same stem seen in Peq. *gĕkíwŭ* "he is drunk" (Prince and Speck, *Anthrop.*, V, p. 206), but it also occurs in Del. *ki kakiwus* "thou art drunk."

Kănúkwŏk "private parts," a plural of *kinkâi* (*q. v.*), is probably a N. E. loanword from the same stem as Natick *kinukkinum* "he mixes, mingles"; cf. Nat. *kenugke* "among." In modern Peq. *kănúkĭ* "privates."

Kĭnkâi, given by Harris as "anus," undoubtedly means either "*membrum virile*" or "*pudendum femineæ*," *i. e.* "the mixer." It seems to be the singular of *kănúkwŏk*, *q. v.*

Kwŏn "yes" is undoubtedly identical with Del. *gohan* "yes" (Brinton, *Lenâpe Dict.*, p. 45, 2).

Mâmîtŭkkŭ "devil" is a corruption of *mânîtŭkkŭ* "he is the (evil) spirit." Note in Natick *mattanitoog* "devils." In Del. *manito* is the regular word for "spirit, God"; cf. Aben. *madahôdo* "devil"; Peq. *muwundo* "God."

Mânuk is a very interesting survival of a New England loanword, *i. e.*, from Nat. *monak* "an English coat, a petticoat"; cf. RW. *maunek* "a European garment" (see *Natick Dict.*, p. 266).

Nîskâhíkĭăn must stand for *mîskâhíkĭăn* "cider" which is a derivative from Del. *masgichlen* "May apple". (*Len. Dict.*, p. 74, 19).

N'pûmîâs is translated by Harris "barn," but is clearly a form of Del. *nibahwi*, *i. e.*=*n'pûwîâs* "during the night"; cf. the Del. *nibahwi* and Aben. *nibôiwi* "in the night-time."

Nŭnŭ'pâ "lift up" must be a reduplicated form of Del. *nipachton* "raise up." I think the guttural breathing should have been on the third syllable, *i. e.*, *nŭnŭpâ'*.

Rŭtîg "crushed corn"; Peq. *yŏkĕg*; Nat. *nuhkik*, lit. "some-

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terminate vowel similar to a short *ĕ*. The consonants have the same values as in English, except *š*=*sh* and ', which is a soft rough breathing like the Arabic medial *h*. In the Abenaki the *ô* is a nasal as in French *on* in *mon*. The Natick and Narragansett words are quoted in the English system which was followed by Eliot and Roger Williams, while the Delaware material is given in the German notation, following the usage of Brinton's Lenâpe Dictionary.

thing softened," according to Eliot, "flour." This word appears as *rucat* in the old New Jersey Lenâpe trading idiom. Cf. Aben. *nokhigan* "flour"; Del. *loken*, from the stem *lokenummen* "smash up, crush." Note that *r*, *y* and *n* interchange in the N. E. Algic dialects; cf. Nat. *nût*, Quiripi *rût* and Peq. *yût* (Stiles *yewt*) "fire" (see *s. v. škûkăríš*).

Šămút "tripe" is evidently the same stem as Del. *schameu* "greasy," *Len. Dict.*, p. 126, 9.

Škûkăríš "snake" is a curious formation. It must of course be from *škûk* "snake"; Aben. *skog*; Nat. *askûk*; RW. *askug*; Morton N. E. Canaan *ascowke*; Peq. *skoogs* (with diminutive *-s*); Del. *achguk*. The *-riš* ending is difficult. It probably stands for *-niš*, *i. e. škûkăníš* "a little snake," as distinct from "a serpent," with intercalated *n*. For interchange of *n* and *r* see *s. v. rúlĭg*.

Špúti, given by Harris as "buttocks," really means "anus." This is the same word as Del. *saputti* (Zeisberger), *Len. Dict.*, p. 124, 16.

Skwâ "woman"; Nat. *squaas*; RW. *squaw*; Del. *ochqueu*, *okhqueh*; the original stem meant "prepuce." This is a well-known Eastern word, but appears only as an ending in Abenaki, as in *kinjames-iskwa* "queen," from *kinjamés* "king" (=King James).

Sû seems to me to be a particle in the possible combination *k'sûkâgîtínôn* "we (incl.) shall drink." It may have a cohortative force.

Súkkŭtăš "succotash" is a well-known N. E. word. Cf. RW. *m'sickquatash* "something beaten up," from *m'sukquttahhash* "the things (inan. pl.) beaten to pieces." *Sukquttahham* "he beats it to pieces." *Súkkŭtăš* is plainly a loanword in the Skaghticoke dialect.

Tăpásŭk, given by Harris as "hotel," probably stands for *nîtâpèsŭk* "my friends" (dim. *-s*). Cf. Aben. *nidôba*, Penobscot *nidâbe*, Pass. *nítăp* "my friend."

Típî "devil" is probably a Pequot loanword from Peq. *dibî* "devil," cf. Prince and Speck, *Anthrop.*, V, 203. The Del. word for "spirit" is *tschipey*, cf. Aben. *chibai*. *Tîpî* in Skaghticoke may, however, stand for Del. *tschipi* "strange," the same stem as *tschipey* "spirit."

Túlĭpás "turtle" is evidently a diminutive (*-s*) from *túlĭpâ*;

cf. Del. *tulpe*, Aben. *tolba* "turtle"; Nat. *tunuppasog* "tortoises."

Wĭnnix "white man," cognitive with Aben. *awanoch*, now used for "Canadian Frenchman"; Pas. *wenoch* "white man." Cf. Peq. Stiles *waunuxuk* "white men"; Nat. *awaunagessuck*, *Natick Dict.*, p. 253. The word is a derivative from the indefinite pronoun seen in Del. *awwen*, Aben. *awasi*, Penobscot *awani*, Munsee *awaun*, Pass. *wen*, "who, someone."

Wĭchiwin "come along" and not "hurry up," as Harris gives it. Cf. Del. *witschewan*, Aben. *wijowi* "come along with me," etc. See *s.v.* *tĭpásik* and *kigitĭwin*.

J. Dyneley Prince.

Stated Meeting, November 20, 1903.

President Smith in the Chair.

The following papers were read:

"The Testimony of the Huacos (Mummy-grave) Potteries of Old Peru," by Albert S. Ashmead. (See page 378.)

"On a Geological Tour to Labrador," by Prof. Amos P. Brown.

Stated Meeting, December 4, 1903.

President Smith in the Chair.

The decease of the following members was announced:

Dr. Charles Schäffer, at Philadelphia, on November 23, æt. 66 years.

Prof. Alphonse François Renard, at Brussels, on July 9, æt. 61 years.

Mr. Henry Carey Baird made some remarks on "The Alaska Frontier."

Prof. Percival Lowell read a paper on "The Cartouches of Mars," which was discussed by Prof. Haupt, Prof. Conklin, Mr. Goodwin, Prof. Doolittle, Prof. Ernest W. Brown and Prof. Heilprin.

THE CARTOUCHES OF MARS.

BY PERCIVAL LOWELL.

(*Read December 4, 1903.*)

That changes take place upon the surface of Mars is manifest to anyone who has given the planet prolonged study. Not only do the polar caps wax and wane with regular rhythm, but the dark markings with which the disk is diversified deepen in tone or fade away as the months succeed each other. The phenomena known as the "canals" are likewise subject to transformation. At times they are conspicuous; at times invisible. And what is yet more striking, each canal has its own times and seasons, its exits and its entrances. What dates the one does not date its neighbor; and still less its antipodes. The Ganges will be seen when the Titan is invisible and the Titan be evident when the Ganges can scarcely be made out.

Particular "canals" are not sole instances of such change. On occasion "canals" in whole regions appear to be blotted out. The most careful scrutiny fails to detect them, though distance be at its minimum and definition at its best. Yet before or after, under conditions much less favorable, the region stands out peopled with lines. Even the strongest and best known of these strange pencilings seem at certain seasons but wan ghosts of their usual selves. As for their more tenuous companions, it almost taxes faith to believe that they can ever have existed at all.

In order to discover what, if any, law underlay these shifting phenomena, I bethought me some two years ago of deducing from my drawings the percentage of visibility of given markings at intervals during an opposition, and of then collating the results. The great number of drawings at my disposal at once suggested this method and increased its trustworthiness, since the accuracy of a percentage heightens with the number that go to make it up.

To get the percentage I had recourse to the following plan. Taking the mean longitude of the marking from the map, I considered all the drawings which, from the longitude of their centres, might be expected to show the marking within certain zones from the central meridian, and then noted the appearance or non-appearance in each of the marking in question. Three such

zones I thought it best to take—those from the centre to 20° out on either side of it; next, those from 20° to 40° out; and last those from 40° to 60° away. This tripartite arrangement had the advantage, which indeed was the reason of its adoption, of furnishing comparison between a marking's visibility at different distances from the centre of the disk. And I may say in passing—for the subject will occupy another paper—that these relative visibilities came out in accordance with what realities on the planet's surface would show.

Were the disk always full the application would be simple and forthright. Being presented generally with a phase, certain corrections have first to be introduced. Since the illumination degrades from the point under the sun out to the terminator where it ceases altogether, a marking from this cause alone tends to disappear as it nears that boundary, and indeed within a certain distance of the night-line can never be seen at all. As such *terra non* I took empirically a zone 25° in from the terminator, such being from my observations the mean value of the semi-obliterated area. Subsequent calculation shows that this is about the value needed to equalize the chances of detection in the three pair of zones mentioned above when all the factors of position conducing to visibility are taken into account.

Convenient epochs for testing the visibility of a canal were self-offered by its several presentations. A presentation of any part of the planet is the occasion of the presentment of that part to an observer upon the earth. As Mars takes forty minutes longer to rotate than our own globe, its longitudes lose on the average 9°.6 a day in coming to the disk's meridian. In consequence of thus slowly falling behind time they complete an apparent backward revolution in about 38 days (from 37 to 41 days), since 9°.6 goes into 360° some thirty-eight times. After the lapse of this period, the two planets again show the same face to each other at the same hour. For a third of the time, therefore, the marking is well placed for observation; for the other two-thirds, it is either not to be seen because the planet is below the horizon or practically invisible because the planet is not high enough up. Thus the presentations make natural epochs for comparing a marking with itself and noting any change in aspect it may have undergone in the interval.

The data were furnished by the drawings. In the present inquiry these consisted of those made by me at the opposition of 1903

just passed, 375 complete ones in all. They date from January 21 to July 26, inclusive, and were divided by months as follows:

January	18	
February	48	
March	49	
April	70	+ 1 unfinished.
May	63	
June	70	
July	57	+ 1 unfinished.
	375	+ 2 unfinished.

Sketches of particular parts are not included in the list, as being unfit for comparison purposes.

The principle I adopted in making the drawings was that of momentary representation. My object in each was not so much an exhaustive map as an instantaneous photograph. From ten to twenty minutes only was the time allotted to each. In that period the shift of the longitudes is not enough substantially to change the degree of visibility of a marking and thus to make of the drawing a composite picture.

Eighty-five canals were examined for presence or absence in these drawings. The average number of times a canal might have been seen, had it been sufficiently conspicuous, proved to be about one hundred. The number of times it actually was seen varied with the particular canal, some canals being but rarely detected, others being almost continuously visible. From the above it follows that eight thousand five hundred separate examinations for the visibility or non-visibility of the canals had to be made in all; an undertaking of some length, but adding proportionately to the trustworthiness of the result.

For getting the percentage visibility of a canal at any presentation it seemed on the whole best to consider all three of the above pair of zones together, or, in other words, the percentage of visibility within 60° of the central meridian, limited as above described toward the terminator. Any other pair of zones might have been used with equal correctness, but the greater number of determinations got from considering all three together commended itself for its increased accuracy.

The percentages thus obtained proved sufficiently suggestive, even before any corrections had been applied. To give them,

however, their full import two corrections had in rigor to be taken into account: one for the varying distance of the planet and the other for the varying quality of the seeing. At the several presentations the planet was not at the same distance from the Earth. Now distance affects the visibility of a marking by altering its size. If the markings be large, their apparent size decreases as the square of the distance. If, as in the case with the "canals," they have length without width, we may take them as of one dimension. For beyond a certain length increase of that quantity does not seriously affect the visibility. Their width, however, although unrecognizable as such, improves their chance of being seen in the direct ratio of the planet's approach.

Now if we take the chance that a canal of twice the width of a given one is twice as likely to be made out, we may regard it as the inverse of the relative chance of commission of twice a given error of observation. We may then use the areas bounded by the curve of probability, with the width of the canals taken for abscissæ, respectively as the measures of the likelihood of detection in the two cases, since these areas include all the chances of seeing a canal of the given width. By taking the area from the central ordinate of the curve out to where that area shall equal the percentage of visibility shown at a given distance, then multiplying the ordinate there found by the inverse ratio of the given distance of the planet at the time to some fixed distance taken as standard, and then finding the area corresponding to this last ordinate, we shall get the percentage at the standard distance. It will be noted that on this principle, as the planet approaches the Earth the percentage of visibility increases gradually to unity, that is certainty of detection if the object exist at the time, since the area enclosed by the curve of probability approaches unity as the abscissa is indefinitely increased. For standard distance I took that of the planet's nearest approach to us during the opposition, when its disk subtended 14".6 of arc. On this principle have been computed the corrections for distance.

The correction for the seeing was got in the following way. The seeing at the time of each drawing was entered in the course of observation by the side of the drawing, together with all the other marginal notes. By taking the mean of these values for all the drawings which entered into the determination of the percentage visibility of a given canal at a given presentation, we get the mean seeing under which it was observed. The correction needed in

consequence was then applied to the curves of visibility as now to be described.

Using the percentages of visibility as ordinates and the times before and after the summer solstice of the planet's northern hemisphere as abscissæ, I plotted the resulting determinations and connected the points so found by a smooth curve. These curves may be called the cartouches of the canals, since they are their distinctive sign-manuals. Each portrays on its face the varying visibility of its canal during the time that it was under observation, but it masks much more. Were the canal intrinsically unchangeable, its curve or cartouche would be a straight line, since corrections for all extrinsic causes of apparent variability have already been applied. Its cartouche would be a line parallel to the axis of abscissæ and at a distance from it proportionate to the canal's strength. On the other hand, any intrinsic change in the canal reveals itself at once by a departure from a straight line. If the canal be for any reason augmenting, its curve will rise; if it be dwindling, the curve must fall. Thus the curves or cartouches tell us not only of the apparent change in visibility but of the real change in development during examination.

On scrutinizing the cartouches the first point noticeable is the well-nigh total absence of straight lines among them. There are but two or three instances throughout the eighty-five. Thus the great majority of the canals were, during the time they were under observation, in a state of flux. For the quiescence of the remaining few we shall a little later in the paper be able to assign a probable cause.

It is next to be noticed that opposition fell not far from the centre longitudinally of the curves, and the time of the planet's nearest approach to the Earth still nearer the middle, since the first of these events happened on the 30th of March, the second on the 3d of April. The summer solstice occurred earlier, on February 28. Another epoch worthy of regard is the date of the first frost in the Arctic regions. This, as explained elsewhere (*Lowell Observatory Bulletin*, No. 1), took place 126 days after the northern summer solstice. It is indicated in the first diagram by a dotted line.

On casting one's eye down the list of cartouches arranged alphabetically, no order or law is apparent. Some canals had their minimum early, some late, according seemingly to their own personal peculiarity. But if now we seek some natural order and

take the latitude as a probable criterion, we shall suddenly be aware of a very different state of things. As the canals are not points but lines, we must select for purposes of precision some point in them as their distinctive latitude and longitude. Their mean point, or more properly the mean of all their points, has therefore been taken in each case, since it is with mean values that we find ourselves concerned. On this principle we may classify the canals by zones of latitude, advancing down the disk from the north polar cap. The canals were therefore ticketed and arranged according to the following zones:

Arctic zone, containing the canals whose mean latitude lay between					86°N.–65°N.
Sub-Arctic zone,	"	"	"	"	65°N.–50°N.
North Temperate zone,	"	"	"	"	50°N.–35°N.
North Sub-Tropic zone,	"	"	"	"	35°N.–25°N.
North Tropic zone,	"	"	"	"	25°N.–10°N.
North Equatorial zone,	"	"	"	"	10°N.–0°
South Equatorial zone,	"	"	"	"	0° –10°S.
South Tropic zone,	"	"	"	"	10°S.–25°S.
South Sub-Tropic zone,	"	"	"	"	25°S.–35°S.

86°N. was taken as starting-point because of the coming down of the north polar cap to about this latitude throughout the course of the observations. On the other hand the lowest zone extends only to 35°S., because, owing to the tilt of the north pole of the planet toward the earth, a tilt which ranged between 21°.1 and 25°.9 during the same period, the farthest observable canal south had 27°S. for its mid-point. The date at which each canal was at its minimum visibility is shown in the following list:

TIME OF MINIMUM DEVELOPMENT OF CANALS.

Arctic Canals—86°–65° Lat. North.

Lat. N.	Name.	No. of Canals.	Days After Summer Solstice.		
78°	Ceraunius N.		4		
75°	Sirenius N.		6		
74°	W. Kison.		0		
72°	E. Kison.		0		
71°	Jaxartes.			—3	
69°	Rhizius.			—9	
65°	Hades.		2		
		7	0		Mean 0

Sub-Arctic Canals—65°–50° Lat. North.

Lat. N.	Name.	No. of Canals.	Days After Summer Solstice.	
64°	Syrgis		11	
63°	Empetis		14	
62°	Pierius		18	
58°	Callirrhoe		10	
57°	Singames		15	
52°	Jomanes		12	
		6	80	Mean 13

North Temperate Canals—50°–35° Lat. North.

49°	Arnon		7	
49°	Dis N		24	
47°	Ceraunius S		24	
42°	Halex		25	
42°	Styx		28	
42°	Udon		20	
36°	Sirenius Middle		25	
		7	153	Mean 22

North Sub-Tropic Canals—35°–25° Lat. North.

35°	Gihon II		59*	
34°	Isiacum		53*	
33°	Brontes N		27	
33°	Titan N		30	
32°	Britannia		20	
32°	Nasamon		43	
30°	Sitacus N			—9*
28°	Dis S		29	
28°	Nilokeras		35	
28°	Phison N		42	
27°	Euphrates N		42	
		11	371	Mean 34
				Mean *33

North Tropic Canals—25°–10° Lat. North.

25°	Phrixus		42
25°	Pyriphlegethon N		34
23°	Djihoun		43
23°	Jamuna N		47
23°	Libycum		54

* Denotes a canal *extra ordinem*, which is omitted in the starred mean.

Lat. N.	Name.	No. of Canals.	Days After Summer Solstice.		
22°	Acheron		35		
22°	Indus		64*		
22°	Nilokeras II		42		
21°	Lethes		44		
21°	Thoth		25*		
20°	Hiddekel		55		
20°	Tamyras N				
19°	Oxus		42		
19°	Uranius		28		
18°	Is		40		
17°	Erebus		31		
16°	Gihon I		50		
16°	Sitacus S			—14*	
15°	Amenthes		67*		
15°	Apis		18*		
12°	Adamas		46		
11°	Hydaspes		45		
		—	—		
		22	838		Mean 40
					Mean *42

North Equatorial Canals—10°–0° Lat. North.

10°	Gigas N		46		
8°	Orcus		50		
7°	Phœnix		43		
6°	Cerberus N		49		
5°	Euphrates S		48		
5°	Phison S		47		
4°	Chryssorrhoas		42		
4°	Iris		47		
3°	Nepenthes		25*		
2°	Triton		26*		
0°	Fortunæ		46		
0°	Ganges		47		
		—	—		
		12	516		Mean 43
					Mean *47

South Equatorial Canals—0°–10° Lat. South.

Lat. S.	Name.	No. of Canals.	Days After Summer Solstice.
0°	Brontes S		50
1°	Clitumnus		52
2°	Pyriphlegethon S		54

Lat. S.	Name.	No. of Canals.	Days After Summer Solstice.	
2°	Titan S.		60	
4°	Jamuna S.		53	
5°	Cerberus S.		54	
5°	Tartarus		63	
5°	Ulysses		71	
6°	Læstrygon		52	
7°	Cyclops		46	
8°	Orosines		58	
10°	Ausonium		66	
10°	Dosaron		56	
		13	735	Mean 57

South Tropic Canals—10°–25° Lat. South.

12°	Erymanthus		54	
12°	Gigas S.		54	
12°	Sirenius S.		63	
15°	Tithonius		73	
17°	Dargamanes		76	
17°	Elison		68	
18°	Aurum		73	
25°	Deucalion		80	
		8	541	Mean 68

South Sub-Tropic Canals—25°–35° Lat. South.

27°	Nectar	1	95	Mean 95

Of the eighty-five canals the number falling into each zone respectively was as follows:

Arctic zone	7	Canals.
Sub-Arctic zone	6	"
North Temperate zone	7	"
North Sub-Tropic zone	10	"
North Tropic zone	22	"
North Equatorial zone	12	"
South Equatorial zone	13	"
South Tropic zone	7	"
South Sub-Tropic zone	1	"
	85	

Naturally the canals on a globe are more numerous near the equa-

tor. Why the north tropic ones are more numerous than the south tropic in the list we shall see later.

Taking now the position in time of the minimum value of the curve of each canal within a given zone, and then determining the mean minimum for all the canals in that zone, we find as follows;

	Mean Minimum, in days after the Summer Solstice.	Or Exclusive of Starred Canals.
Arctic zone	0	0*
Sub Arctic zone	13	13*
North Temperate zone	22	22*
North Tropic zone	34	33*
North Sub-Tropic zone	40	42*
North Equatorial zone	43	47*
South Equatorial zone	56	56*
South Tropic zone	68	68*
South Sub-Tropic zone	95	95*

Disclosed stands a steady progression in the time of minimum development of the canals as we travel from the neighborhood of the polar cap to the equator. The orderly advance becomes even more noticeable when certain canals which appear to contain mistakes or misidentifications or mutual exchanges of visibility are eliminated. Such seem to be the Amenthes-Thoth-Nepenthes-Triton system, in which just after opposition the Thoth-Nepenthes-Triton apparently replaced the Amenthes, and then died down later as if nothing out of order had happened. The Indus and the Gihon II, or that part of the Gihon north of the Deuteronilus, are not impossibly another case of interchange. The two Sitacus and the Apis may be cases of straight li masked in their earlier presentations by distance and unfavorable seeing. For the out-of-place development of the Isiacum, I am at a loss satisfactorily to account. Omitting the above canals from the count we get the second row of minima, which show a yet closer approach to uniformity of progression. Indeed, if we now plot the mean curves or cartouches of the mean canals at ordinal intervals corresponding to the degrees of latitude at which they occur, we shall find that a straight line will nearly pass through all the points. This is shown in Plate XV, which, based on Mercator's projection, makes of the straight line a curve slightly convex on the advancing side. But what is more remarkable, the progression does not stop at the

equator, but continues on into the planet's southern hemisphere, the sign curvature changing when it crosses the line.

Thus much of canal development the curves definitely state; but we may infer more.

Whatever constitute the canals, it is evident that their development proceeds from the pole down the disk, and, furthermore, that it advances over the surface at a fairly regular rate. It starts at the summer solstice; that is it follows the melting of the polar cap. This suggests the source of the quickening. In consequence of the water then let loose the "canals" come into being. That this can be due to a bodily transference of matter, the water in question, seems negatived by the area concerned. More darkened area is gained than is lost. But this is not an easy point to be sure of. More forthright is the negativing of such transference by the time taken. Water would make its presence felt long before the actual darkening takes place. For at the latitude of 75°, the mid-latitude of the Arctic canals, the darkening begins on the day of the summer solstice, which is considerably after the date of the most rapid melting of the cap.

But though water directly does not account for the phenomenon, water indirectly does. A quickening to growth of some kind would produce the counterpart of what we see. And these statistics furnish us with a key to its character. It is a seasonal change, but a little consideration will suffice to show us that it is quite unlike in behavior the seasonal change we know on earth.

Could we get off our earth and view it from the standpoint of space we should mark, with the advent of spring, a wave of verdure sweep over its face. If absence of cloud permitted of an unveiled view this flush of waking from its winter's sleep would be evident, and could be watched and followed as it crept higher and higher up the parallels. Starting from the equator shortly after the sun turned north, it too would travel northward toward the pole. Here, then, we should mark, much as we mark it on Mars, a wave of darkening, the blue-green of vegetation superposed upon the ochre of ground, spreading over the planet's surface; but the two would differ, the mundane and the Martian vegetal awakening, in one fundamental respect—the earthly wave travels from equator to pole; the Arean from pole to equator. Clearly the causes compelling them differ. Yet are they both seasonal in character. To what then is the difference due? To the presence or absence of moisture.

Two things are necessary to the begetting of vegetal life, the raw material and the reacting agent. Oxygen, nitrogen, water and a few salts make up the first, the sun does the second. Unless both be present the quickening into life never comes. Now the one may be there and the other not, or the other there and the one not. On earth the material including water is, except in certain destitute spots, always present; the sun it is that periodically withdraws. Observant upon the coming of the sun is then the annual quickening of vegetal life. On Mars, on the other hand, it is the water that is lacking. This we know from many other phenomena the disk presents. There is no surface water there save for what comes from the periodic thawing of the polar caps. Vegetation cannot start in any quantity until this water reaches it. Vegetal change, therefore, on Mars should start from the pole and travel equatorward. On the earth it should do the precise opposite. Now such is exactly what the curves of visibility of the canals exhibit. Timed primarily not to the coming of the sun but to the coming of the water, vegetal life there follows not the former up the latitudes but the latter down the disk. We may conclude then that the canals are strips of vegetation fed by water released from the polar cap.

The two curves of phenological quickening, the mundane and the Martian, are shown in Plates XVI and XVII. The stars mark the dead-points at successive latitudes.

We now come to a deduction from the evidence before us even more startlingly pregnant of information. Glancing at Plate XV of the mean canals, we see that the quickening proceeds rapidly and very nearly if not quite uniformly down the disk. It takes the darkening only fifty days to descend from the seventy-first parallel to the equator, a journey of some 2600 miles. This means a speed of fifty-three miles a day, or two and two-tenths miles an hour. And it does this in the face of gravity. For the spheroidal flattening of Mars, $\frac{1}{191}$ of the polar diameter, shows that the figure of the planet is in fluid equilibrium under the axial rotation. A particle of water, therefore, would know no inclination to move from where it initially was. Of its own accord it would not flow toward the equator. And as it does flow toward the equator, and at a remarkably steady progression too, the inference seems unavoidable that it must be carried thither by artificial means. We are thus led to an artificial origin and maintenance of the markings called canals, and one which in essence justifies that appellative. Nor do I see any escape from the deduction.

This idea is strengthened by another circumstance connected with the development exhibited by the table. The progress of the minima, which betoken the later and later starting of the quickening down the disk, does not stop at the equator, but advances with fine indifference to that natural limit into the planet's other hemisphere. Now there the physical conditions to affect it are the precise opposite of what they were in the first or northern one. If, therefore, it were due to such cause the action should there be reversed. That it is not shows that we are here face to face with a phenomenon not simply inexplicable on natural laws but absolutely antagonistic to them.

The study here presented leads, then, to three conclusions: (1) The "canals" develop down the disk from material supplied by the melting of the polar cap; the development proceeding across the equator into the planet's other hemisphere. (2) The canals are from their behavior inferably vegetal and (3) of artificial origin.

BOSTON, December 4, 1903.

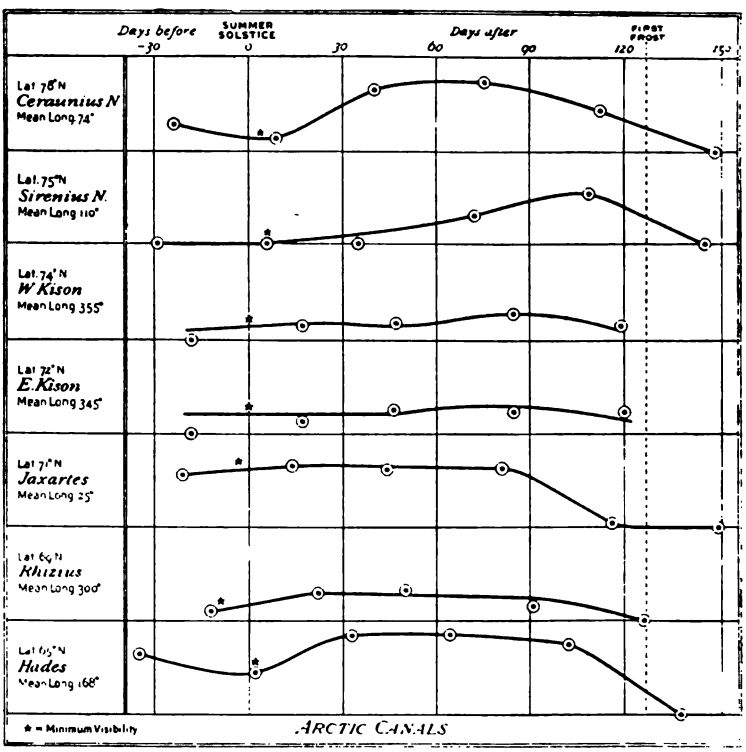

Days before
SUMMER SOLSTICE
Days after
FIRST FROST
-30
0
30
60
90
120
150
Lat 78°N
Cerauntus N
Mean Long 74°
Lat. 75°N
Sirenius N.
Mean Long 110°
Lat. 74°N
W Kison
Mean Long 355°
Lat 72°N
E. Kison
Mean Long 345°
Lat 71°N
Jaxartes
Mean Long 25°
Lat 69°N
Rhizius
Mean Long 300°
Lat 65°N
Hades
Mean Long 168°
* = Minimum Visibility
ARCTIC CANALS

Plate I.

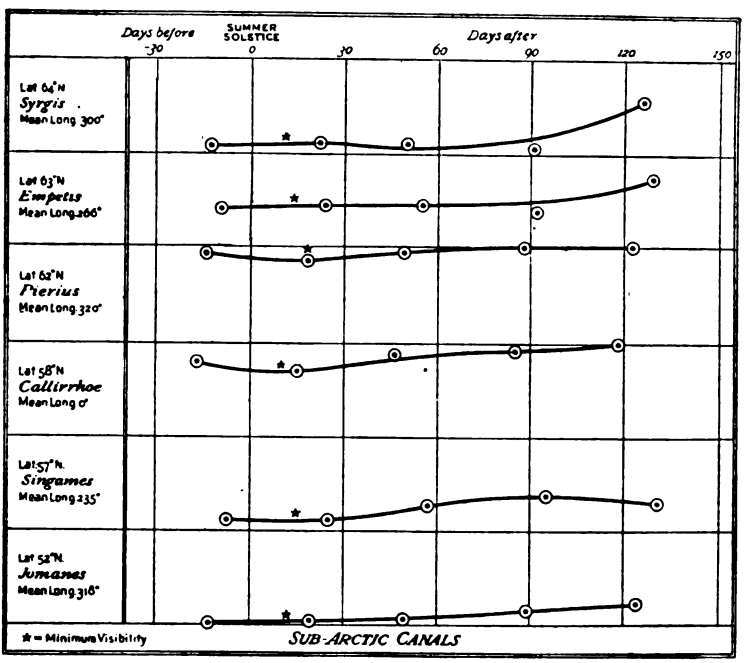

Days before
SUMMER SOLSTICE
Days after
-30
0
30
60
90
120
150
Lat 64°N
Syrtis
Mean Long 300°
Lat 63°N
Empetis
Mean Long 266°
Lat 62°N
Pierius
Mean Long 320°
Lat 58°N
Callirrhoe
Mean Long 0°
Lat 57°N.
Singames
Mean Long 235°
Lat 52°N.
Jumanes
Mean Long 318°
☆ = Minimum Visibility
SUB-ARCTIC CANALS

Plate II.

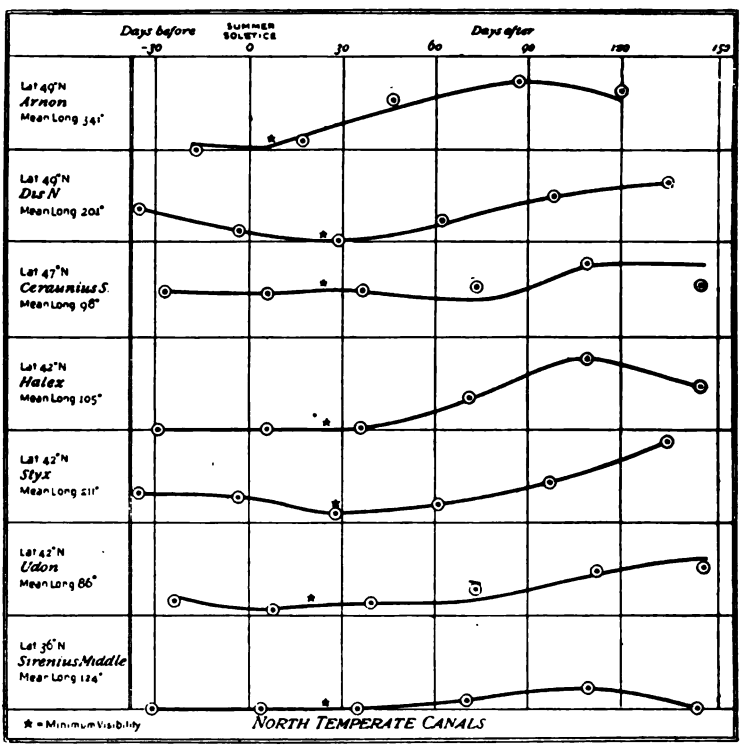

Days before
SUMMER SOLSTICE
Days after
-30
0
30
60
90
120
150
Lat 49°N
Arnon
Mean Long 341°
Lat 49°N
Dis N
Mean Long 201°
Lat 47°N
Ceraunius S.
Mean Long 98°
Lat 42°N
Halex
Mean Long 105°
Lat 42°N
Styx
Mean Long 211°
Lat 42°N
Udon
Mean Long 86°
Lat 36°N
Sirenius Middle
Mean Long 124°
★ = Minimum Visibility
NORTH TEMPERATE CANALS

Plate III.

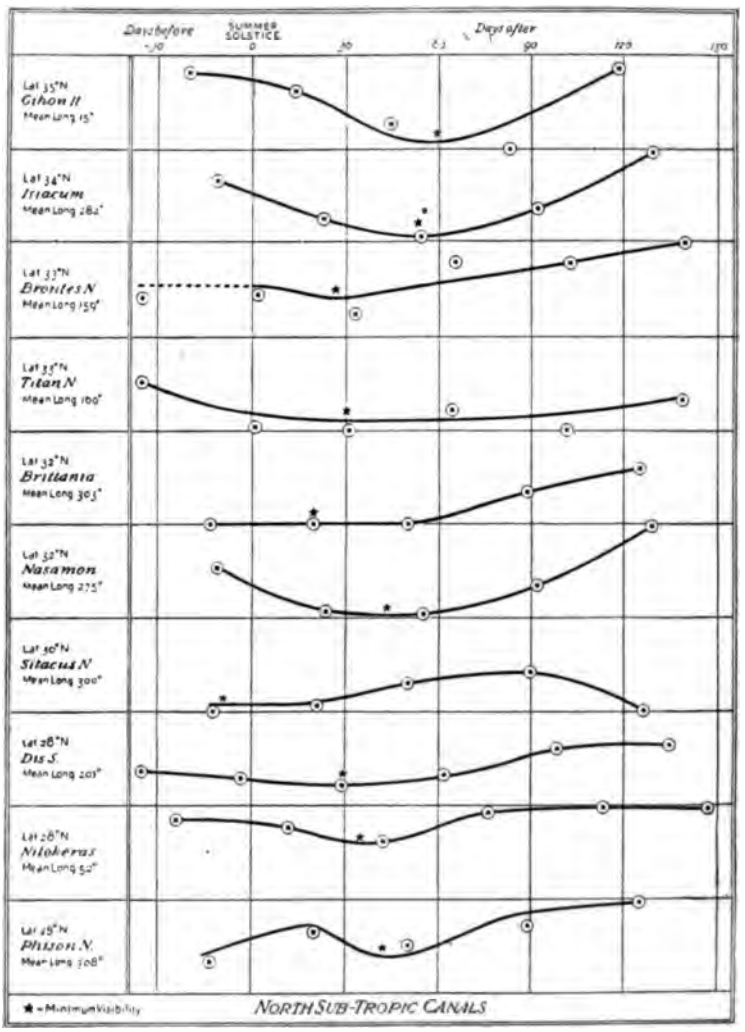


Plate V

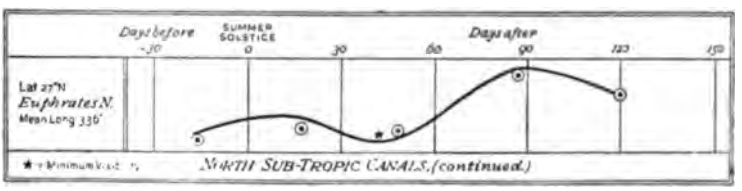


Plate V.

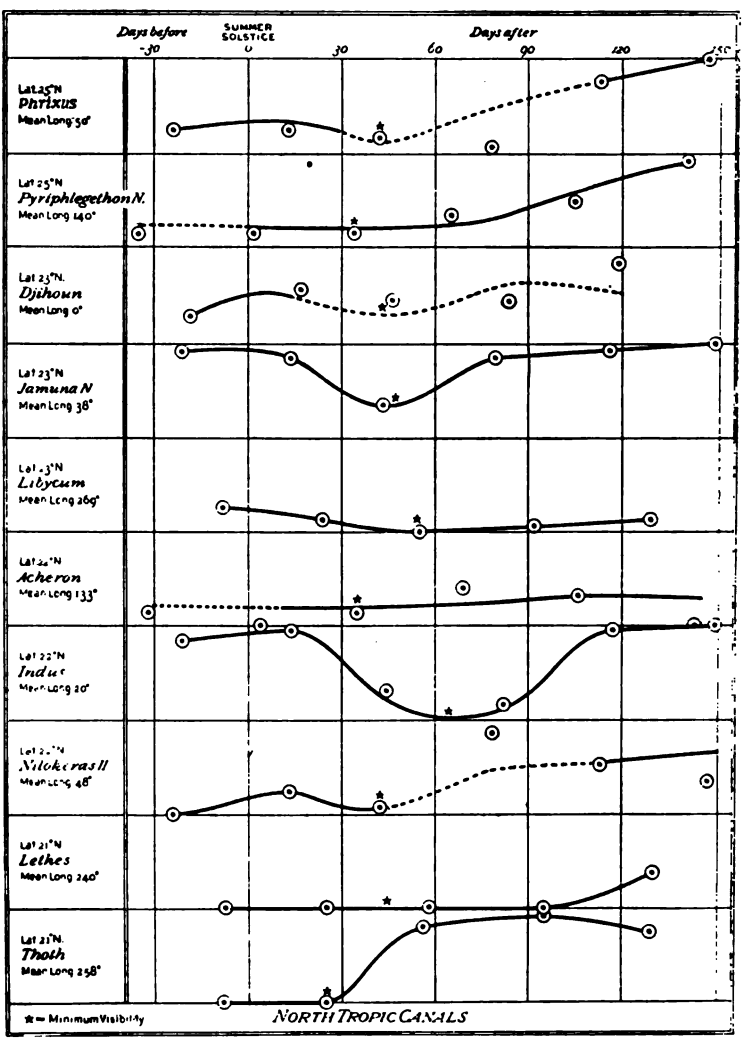


Plate VI.

The broken lines denote such portions of the curves as for certain intrinsic reasons seems the more probable.

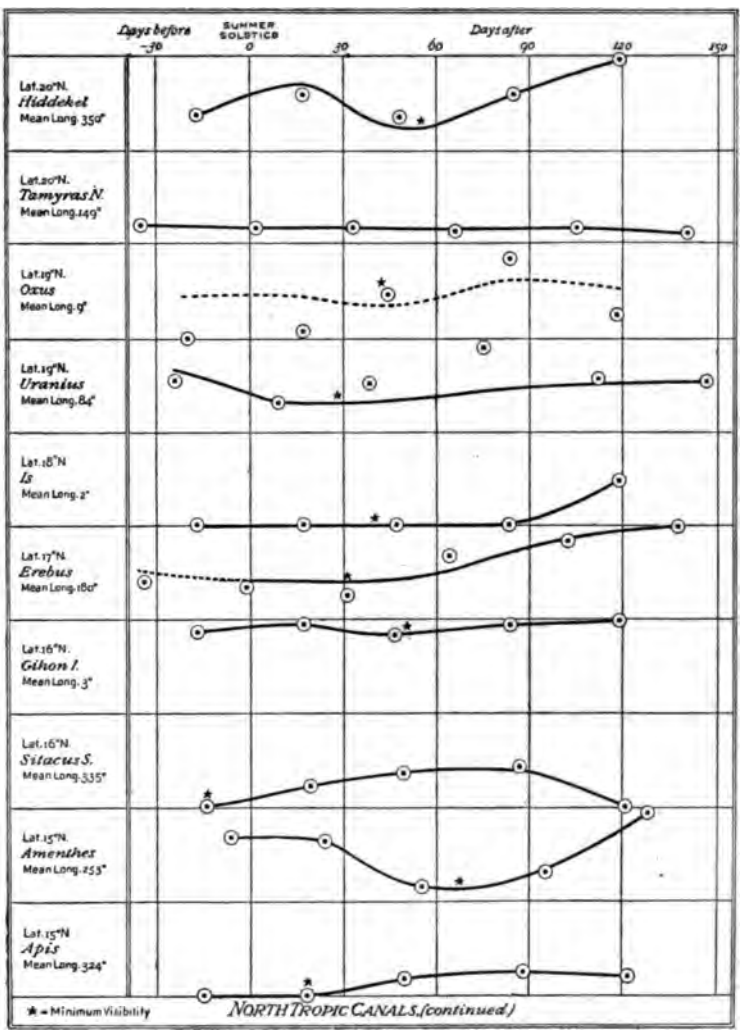


Plate VII.

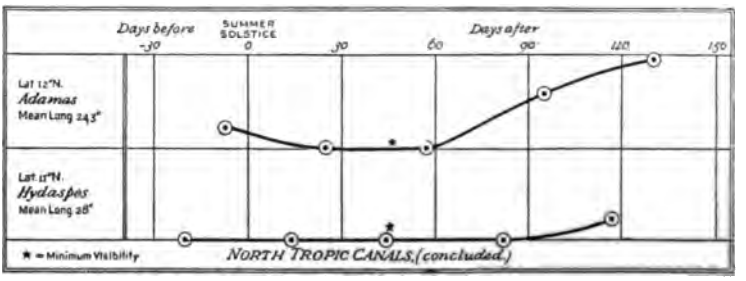


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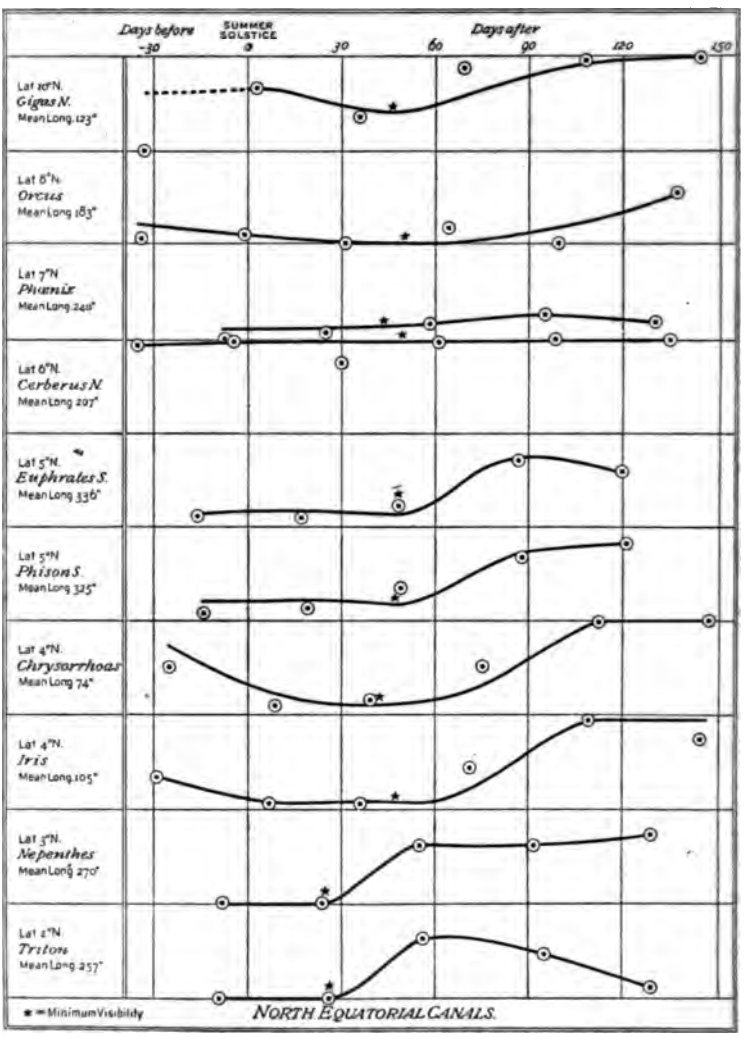


Plate IX.

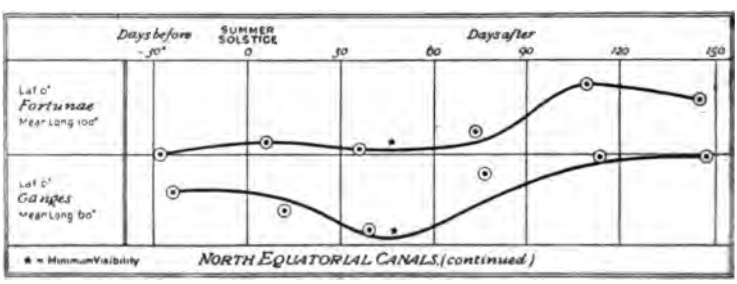


Plate X.

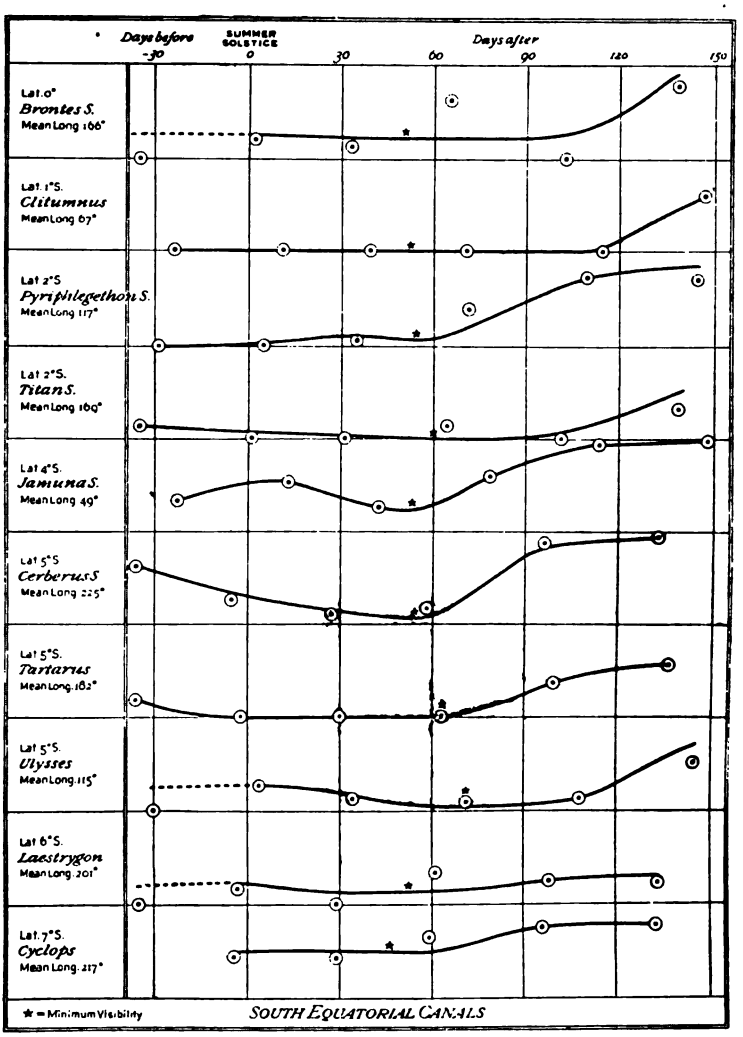

Days before
SUMMER SOLSTICE
Days after
-30
0
30
60
90
120
150
Lat. 0°
Brontes S.
Mean Long. 166°
Lat. 1°S.
Clitumnus
Mean Long. 67°
Lat. 2°S.
Pyriphlegethon S.
Mean Long. 117°
Lat. 2°S.
Titan S.
Mean Long. 169°
Lat. 4°S.
Jamuna S.
Mean Long. 49°
Lat. 5°S.
Cerberus S.
Mean Long. 225°
Lat. 5°S.
Tartarus
Mean Long. 182°
Lat. 5°S.
Ulysses
Mean Long. 115°
Lat. 6°S.
Laestrygon
Mean Long. 201°
Lat. 7°S.
Cyclops
Mean Long. 217°
★ = Minimum Visibility
SOUTH EQUATORIAL CANALS

Plate XI.

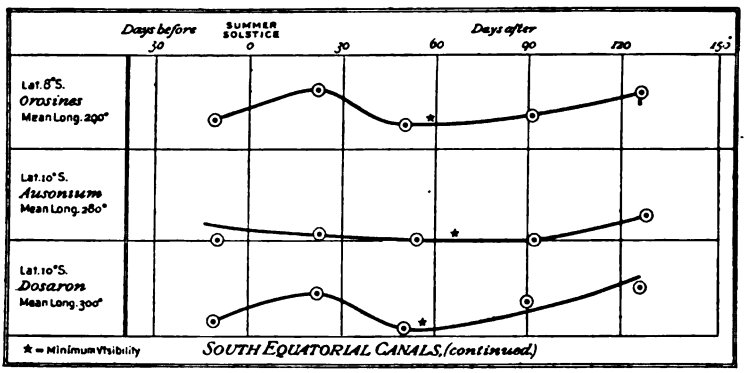


Plate XII.

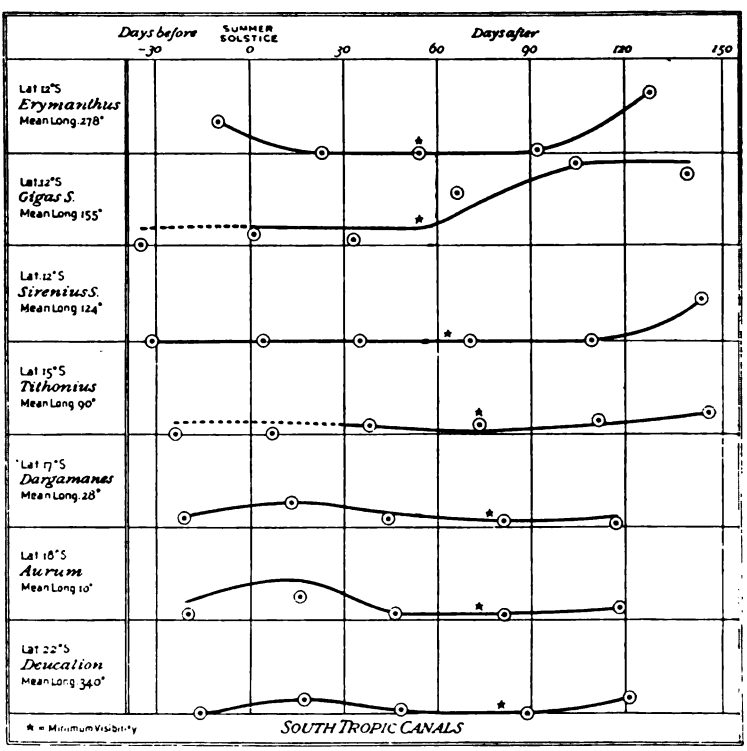


Plate XIII.

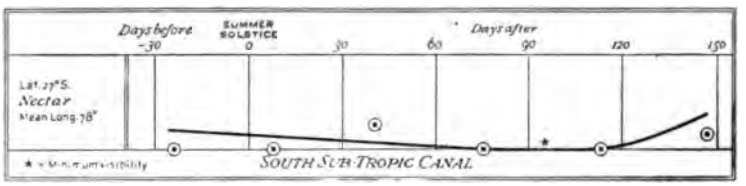


Plate XIV.

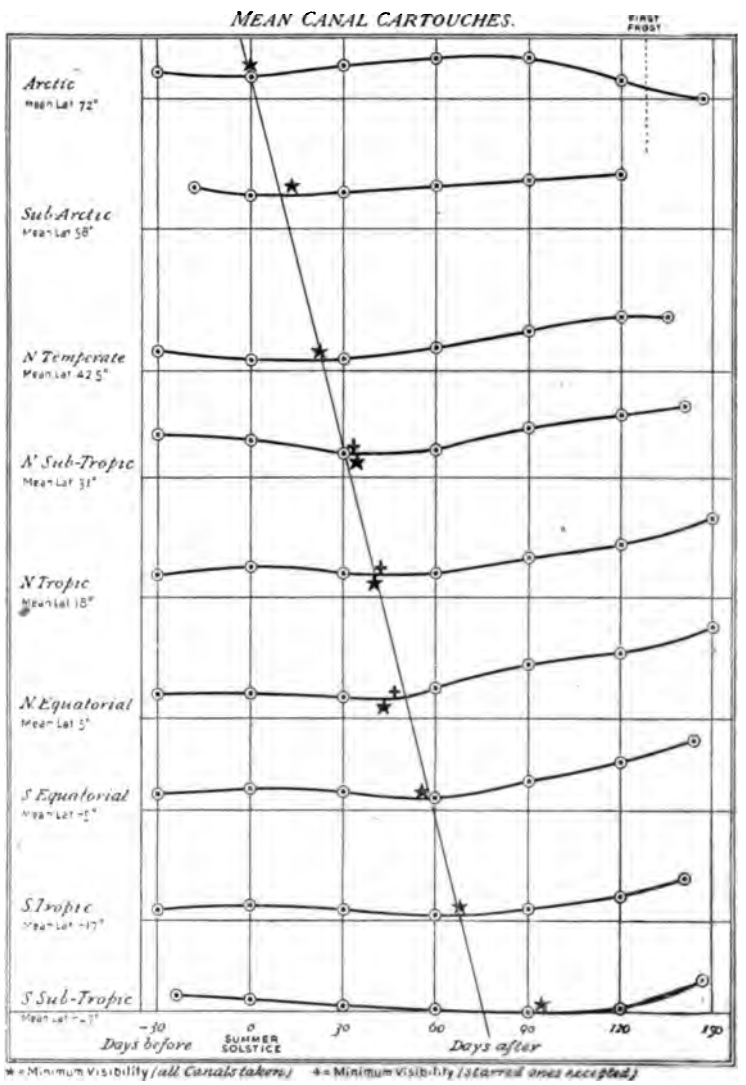
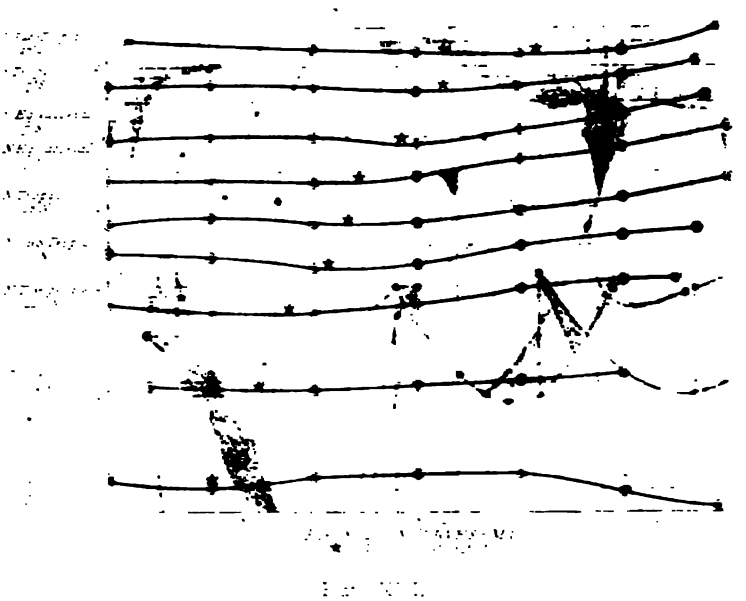


Plate XV.



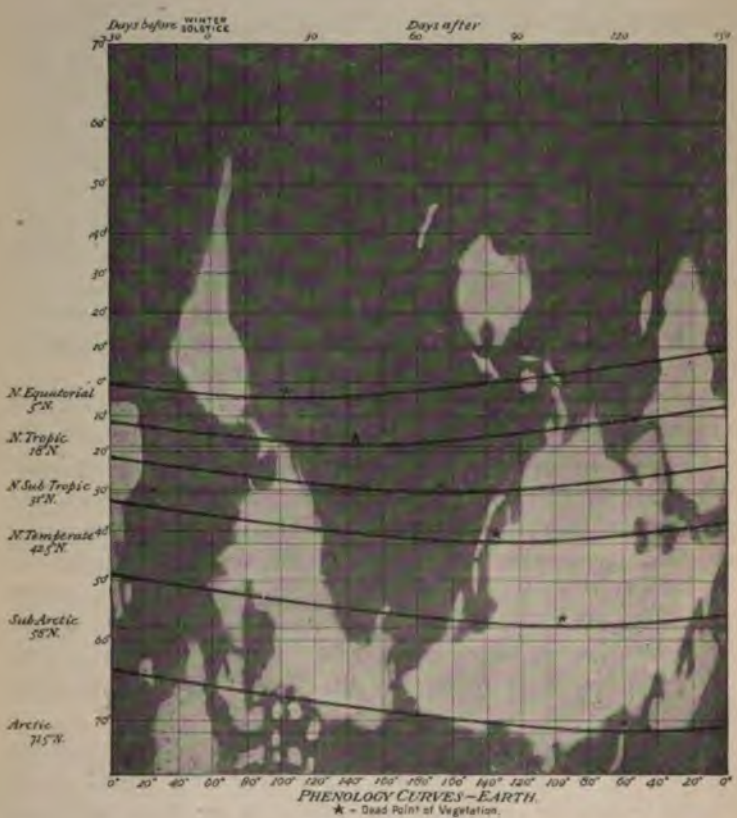

Days before WINTER SOLSTICE
Days after
N. Equatorial 5°N.
N. Tropic 18°N.
N. Sub Tropic 31°N.
N. Temperate 42.5°N.
Sub Arctic 58°N.
Arctic 71.5°N.
PHENOLOGY CURVES—EARTH.
★ = Dead Point of Vegetation.

Plate XVII.

us in his *L'Amour dans l'humanité*, there have been found some wooden figures bearing about the neck a serpent which was believed to devour the body. These images were *idols*, and this representation was the expression, as I defined it, of the disease, syphilis, before those ancients of Peru had a word for it in their language. The serpent is represented in the act of devouring a certain part of the body in a series of the figures preserved in the Museum of the Trocadero. There is also one of these figures in the American Museum in New York.

Here are five of these Peruvian vessels, presented to the Museum of Paris by Mr. Drouillon and derived from Moche. All show in diverse degree some destructive lesions of the upper lip and of the nose.



Figure 1. Peruvian Vase from Moche (Museum of the Trocadéro). The extremity of the nose is destroyed.



Figure 2. Limited destruction of the upper lip.

In the first the extremity of the nose (septum and wings) is destroyed. There is no other alteration. The rest of the nose and the upper lip are intact.

The second subject has undergone a limited destruction of the middle of the upper lip. A portion, in the form of an obtuse angle with its summit bordering on the septum, has disappeared, throwing into view the gums and teeth which remain intact. The borders of the lesion are clean, and appear cicatrized; the nose seems pointed, and the two wings are strongly spread out.



Figure 3. The upper lip is eaten away.



Figure 4. Cicatrization following necrosis of the upper jaw.

The third subject expresses an alteration most grave. The upper lip is devoured, likewise the nose, uncovering the gums, which are red and bleeding.

The teeth are complete, but the end of the nose has disappeared; this is of abnormal shortness and appears too high.

The fourth pottery is even more interesting. There has been necrosis and loss of the superior maxilla, which has undergone a retraction over the inferior. A cicatricial tissue has formed, tight and inextensible, which leaves the teeth uncovered and obstructs the entrance of the nostrils. The lower eyelid of the right eye, held by the cicatricial tissue, leaves uncovered the ocular globe, while that of the left eye is normal.

The last pottery of this series represents a mother, who holds her infant in her arms. In her case also there exists a loss of the upper jaw. But here the nose is destroyed at its root; the extremity, intact, is turned up. This form of nose has been well described by Fournier, the syphilographer of France.

Similar potteries are not rare. They exist likewise in the Museum de la Plata, Argentina, South America. A beautiful collection of photographs of this last Museum is on exhibition at the

ages of the departed, and they would not have made them false. Amputation of hands was never represented on a pot, because artificial hands were necessary to carry the drinking water to the lips. On not one single pot anywhere in the whole Museum world is there represented a mutilated hand or a tuberculated face. This in itself is conclusive evidence that leprosy was *not* pre-Columbian in America.

These potteries of the Trocadero offer more perfect signs yet in favor of syphilis and of lupus representations; those multiple lesions of the nose are characteristic of syphilis, or of syphilis and lupus combined.

If there is any doubt of it, it is not in favor of leprosy but of lupus, as is shown in the subject Fig. 4. Even this subject derived from the Museum de la Plata, with retraction of the skin of the face, might equally be afflicted by lupus.

A last argument is furnished us by an examination of the thousands of pre-Columbian bones of American graves. Not one offers a leprous lesion, as we find them represented in the graves of the cemeteries of the "Madeleines" of France, where are found the little bones of leper hands as if *melted away* to a fine thread, but never so in ancient American graves. Quite a number of the American bones from ancient American graves, undoubtedly pre-Columbian, on the contrary, are syphilitic.

We all must admire the dexterity of those old Peruvian artists, who have given us such good representations of the ulcerative lesions of these diseases.

Besides the evidences of an "eating disease" on the faces of these clay vessels of the graves of Old Peru, there are a number which appear as if the nose and upper lip had been cleanly cut off with a knife.

Here is a photograph of one such, which Prof. Bastian, of the Royal Museum of Berlin, kindly sent me (Fig. 6). There are others with this same exhibit in the Bandelier Collection of the American Museum of Natural History, New York.

Mr. Wilhelm Von den Steinen, to whom the original of this pot belongs, says: "It is from Chimbote. The tip of the nose and the upper lip are destroyed, the cheeks 'flown out' and furrowed with wrinkles or scars." I submitted this photograph, after Prof. Bastian had sent it to me, to Dr. Hansen, of Bergen, Norway (the discoverer of the leper-bacillus), and he replied that "it did not present signs

of leprosy." "There are no tubercles on it," he said, "and no phenomena of anesthesia."

This photograph has always appeared to me as if the person it represents might have been mutilated by a surgeon's knife for lupus.



Figure 6.

Dr. Ugaz, the best authority in Peru to-day on this last-named disease, concludes an interesting article, "Etiologia topografia y tratamiento de la Uta (lupus)," as follows: "Uta (gallico, llaga, Ilianya, tiacarañia, Qquespo Spondyle) of Peru is bacillary tuberculosis, generally localized in the uncovered parts of the skin (tuberculo-derma), and its *only* treatment is endermic and *surgical*." My own conclusion is that this Uta, gallico, llaga, etc. = pre-Columbian lupus (with or without complication with syphilis), is the disease represented on the huacos potteries, for some of those specimens represent the effects of the surgical treatment of that disease, the *cutting off of nose and upper lip*.

It is highly probable that some of the deformations of those ancient Peruvian figures were intended to represent lupus and syphilis combined and not leprosy. For, as I said, Ancon, the pre-Columbian graveyard of Old Peru, was also the place of baths where the "luposos and sarnosos" congregated for *curative* treatment.

Had Ancon been a resort for lepers, somewhere in an European or American Museum we should be able to discover a *mummy* showing loss of fingers or toes, for most lepers are thus mutilated. But,

quite to the contrary, no such disfigurement of pre-Columbian remains up to this time has been found in any Museum of the world. I have searched all over for such and without success. Moreover, had there been lepers in pre-Columbian Peru, they surely would have gone to those baths along with the luposos and syphilitics. Only the syphilitics could have been cured, while the luposos and lepers, being incurable without surgery, would have died there. Thus the absence of leper remains from the graves of Ancon is double proof that leprosy did not exist in pre-Columbian Peru.

In determining in some of these representations of diseases on these ancient potteries what disease each one is, it must not be overlooked that even in the living subject the diagnosis between leprosy, syphilis and lupus is sometimes most confusing to a physician and even to a trained leprologist. This is especially true when the patients belong to degenerate or dying-out races. How much greater then must the difficulty be to determine the identity of one of these diseases whose representation was carved on the face of a small clay image by an artist who was not a medical man. We must observe, moreover, that in the representation of a disease on the clay figure of a man, intended to record what belonged to the corpse, and to be forever buried with it as its "double" or *soul*, the failure to show in that clay figure a mutilation of fingers or toes or tuberculation of face, the most usual deformities of leprosy, should indicate to us that the disease which the handicraftsman had illustrated was not leprosy at all but some other disease.

There is a specimen of ancient Peruvian pottery in the Royal Museums for Ethnology in Berlin which I have figured in the *American Journal of Cutaneous Diseases*. These photographs originally were given to me by Prof. Bastian, of the Berlin Museum. It is the figure of a man, apparently a dwarf, whose skin is covered with tuberculous lumps. The question is, What does it represent? And, more especially, does it afford any proof of the existence of either syphilis or leprosy in ancient Peru? It is quite clear that the artist has copied from some living subject, and we have at any rate offered for our inspection a very early delineation of the disease. This pottery is probably a thousand years old.

Jonathan Hutchinson, F.R.S., of London, to whom I submitted the photograph, argued with me that there is no reason to consider the disease leprosy, for the man is scratching very vigorously and clearly has no anesthesia of the skin, which would belong to him

had he leprosy. His head is thrown back. Nor in the tuberose form of leprosy are the tubercles ever so freely developed on the trunk as is here shown. Mr. Hutchinson believed that the figure represented Molluscum fibrosus, a disease of skin which does not exist in Latin America to-day; and had it existed there in pre-Columbian time, would it not be found in Peru to-day? Besides these objections to Mr Hutchinson's diagnosis there is the upper lip shown to be eaten away, as is so common in the other Peruvian potteries. Molluscum is not essentially pruriginous, but scabies or pediculosis might have been present to account for the itching. To my mind, it is another instance of *lupus representation.*

I have also nine representations of the grave potteries of old Peru. The first is indentical with a huacos pot in the Field Columbian Museum, Chicago, a photograph of which was kindly sent me by Dr. Dorsey, and which I published in my article, "No Evidence in America of Pre-Columbian Leprosy," in the *Canadian Medical and Surgical Journal,* March, 1899 The 4th, 7th and 9th are identical with those of the Bandelier Collection of the American Museum of Natural History, which I published, with permission, in the *Journal of the American Medical Association,* in an article entitled "Pre-Columbian Leprosy," April, May and June, 1895, and in the *Verhandlungen of the Berlin Leper Conference.* The 2d, 3d, 5th, 6th and 8th of these images are representants of lupus and syphilis in their deformations. It should be noticed, as we proceed, that in every case the fingers are represented normally.

As to the question of pre-Columbian origin of these vases, those must be regarded as *certainly* pre-Columbian which have been found with a certain gold ornamentation, the gold brow feather, the exclusive ornament of the Inca family. I have seen these "brow feathers" in the collections in the Ethnological Museum known as the Bässler, formerly belonging to Herr Krätzer, of Lima, and also in the new collection of Mr. Krätzer. Besides some of the images were buried with diseased bones, notably one sent up by Mr. Bandelier, the explorer, from Lake Titicaca, of Peru, to the American Museum of New York, which was dug up along with a pre-Columbian Pachacamac syphilitically *diseased* skull. I took a photograph of this skull to accompany my contribution to the Berlin Leper Conference (article entitled "The Question of Pre-Columbian Leprosy in America, and Photographs of Three Pre-Columbian Skulls"). Dr. Patron, of Lima, and Dr. Manuel A.

Muniz, of the same city of Peru, have studied the subject of these potteries, so far as they relate to leprosy. Dr. Patron says, "Leprosy has remained an unknown thing to the native born of Peru, as is evidenced by the lack of a word for leprosy in the Kechuan and Aymaran languages." When leprosy appeared with the invading Spaniards and negroes, a phrase became necessary to be added to the language. Bertolini, in his dictionary of Aymara, gives for leprosy the word "Caracha," which means "itch." And Gonzales Holguin, in his book on the Ketchua language, defines "Liutlasca Caracha" as "itch."

Dr. Muniz wrote me that "the first introduction of African negroes into Peru was in 1536." "The first negro was with the *thirteen* of the Isle of the Cock before the conquest of Peru. There were maroon negroes in Peru in that same year. The king granted to Pizarro the privilege of importing negroes." These Spaniards and negroes introduced leprosy to Peru. Dr. Patron thinks that the diseases which can produce mutilations like those seen on the pottery are syphilis, boils, verruga-Peruana, or Peruvian warts, a disease with fever and peculiar to Peru (this is described by Odriozala, Paris, 1898, as Maladie de Carrion, for Dr. Carrion, a pupil who died from self-inoculation of it to determine its specific characters), and "Uta" (lupus). The word "Uta" means "to eat away," and would naturally be applied to a disease which destroys the tissues. The disease is called variously in different localities: Gallico ("French Disease"=the Spanish name of syphilis when it first appeared in Spain); llaga, Ilianya, Tiac—Araña and Qquespo. All the best authorities attribute this disease to the sting of insects, or by deposition of their eggs beneath the skin. Insects are especially attracted to the mouths and noses of sleeping persons, and those parts especially would be most liable to be inoculated by such a disease as lupus, which has for its germ the tubercle-bacillus of Koch, for aviary tuberculosis in Peru existed long before human tuberculosis was known. The Indians of the Peruvian Sierras are extraordinarily susceptible to lung tuberculosis directly they are transferred to the coasts, while in altitudinal Andes this phase of this pre-Columbian disease does not appear. Dr. Patron's great remedy to-day for Peruvian lupus is cauterization with the Paquelin battery. In other words, all authorities agree on the cure of it by no other means than the *knife* or by burning it out.

Mr. Bandelier, of the American Museum, in reply to my question whether the Peruvian images labeled Chancan and Chimbote, which he had sent up, were to be considered pre- or post-Columbian, said that some of them were and some were not.

The question of the pre-Columbianism of these pots, which arose when I brought them to the attention of the Berlin Leper Conference, was afterwards thoroughly discussed in the Berliner Gesellschaft für Anthropologie, Ethnologie und Urgeschichte (see *Zeitschrift*, 1897, 1898 and 1899), by eminent *Americanists*, such as Polakowsky, of Berlin; A. Stübel, of Dresden; Reiss, of Berlin; Dr. E. W. Middendorf, Dr. Edward Seler, of Berlin; Dr. Marcus Jiminez de la Espada, of Madrid; Dr. A. Bastian, the Director of the Royal Museums of Berlin; Prof. Virchow, President of the Society; Dr. Carrasquilla, of Bogota; Dr. Lenz and Dr. Lehman-Nitsche, of La Plata Museum, and Von den Steinen, etc. I brought before these eminent and learned gentlemen all the evidence furnished me by Mr. Bandelier and the anthropologists of America. Mr. Bandelier had written me that all his "finds" were



Figure 7.

pre-Columbian, and especially described a huacos pot representing a human amputated foot, which I had described in my original paper. The fact that it was a diseased foot would indicate that it had not been amputated as a punishment "for crime," as Dr.

Carrasquilla, of Colombia, South America, had thought. That it is a disease representation is shown by the toes of the clay figure being elevated from the ground, as if the sole of the foot was greatly swollen. This Pachacamac foot-pot was dug up from a grave twelve feet deep; not a bead nor a piece of glass or copper was ever found in that pre-Columbian burial-ground. This is an indication of pre-Columbianism. Moreover, this pot, which I reproduce here, shows the bone protruding and the flesh cut away, just as would appear on a foot that had been amputated, for the flesh flaps must be thus provided to cover the stump of the leg. Mr. Bandelier wrote me as follows of this peculiarity of the figure: "I think that the figures represented without feet ought to be considered as *amputated*, so that they have nothing to do with the question of leprosy or syphilis."

Certainly a people that could trephine a skull as admirably as these same Incas, as is shown by one photographic specimen sent me from Peru (which I here reproduce for purpose of illustration), could just as well amputate with the stone knife a foot properly (see "Pre-Columbian Surgery," Ashmead, *Univ. Med. Mag.*, 1896).



Figure 8. Trepanation of the Incan Epoch (Squier's skull).

This Fig. 8 shows a trepanation of the Incan epoch: A cranium of Yucay. Nelaton and Broca determined that it belonged to the

indigenous race and that it was ante-mortem. Broca concluded that such an operation was performed for extravasation of blood in the cranium from a number of causes—wounds, punctured fracture, violent inflammation, suppuration, delirium, coma, etc.—just as is done by our surgeons to-day.

I have also pictures of ten huacos potteries of La Plata Museum, Argentina, which Dr. Lehman-Nitsche submitted to me. As will be seen also by a reference to those of the Bandelier Collection of the American Museum, New York, while amputation of the feet is often represented, in not one single pot is there a hand amputated. Dr. Polakowsky raised the point that if these amputations were due to disease there should be representations of amputated hands as well as feet. But he overlooked the important fact that then the soul of the departed could not reach out his hand for the wine or water-bottles which are necessary for his future life in the grave or for his four days of journey to Paradise. The whole intent of putting these little bottles in the grave with the corpse is to keep death from becoming definite. A *handless* soul representation would destroy their religious belief. Therefore, even if the hand of the corpse was amputated, they would put on the image they buried with that corpse, *good hands* to help the individual in the other world.

Dr. Carrasquilla was of opinion that these amputation representations do not treat of disease at all, but of punished criminals; that for little faults they cut off the nose and upper lip, and when they punished "relapsers" they amputated also the feet, for the purpose of hindering them from committing new crimes or to keep them from running away.

Dr. Carrasquilla promised to send documentary proofs of this belief of his, but they were found to be totally insufficient to prove his point. Dr. William Von den Steinen has consulted all the literature of South America, like, for example, the works of Cieza de Leon, of Garcilaso de le Vega, and he has *not* been able to find indications of mutilations that prove that the representations on the clay figures have been produced by punishments which had been applied to the individuals. He believes that they refer to the representations of a disease. Mr. Stübel participated in the same belief. Mr. Bastian and Mr. Middendorf thought that they treated simply of punishments applied to criminals. Mr. Seler believed that leprosy had existed in pre-Columbian *Mexico*, because of the

well-known word "teococolitzli," which was applied to leprosy and *to skin diseases generally!* Mr. Jiminez de la Espada gave the question a new turn, that he did not believe that leprosy nor elephantiasis (its variety) had been of pre-Spanish origin in Peru; there were no documentary proofs known to him which supported such opinion, and he was not in accord with the opinion of Carrasquilla, Bastian and Middendorf, who thought they treated of criminals and beggars. He claimed that they did not apply mutilations of the body as punishment, unless death was intended to follow them, and that there were no beggars at all among the Incans, due to their social order so perfect. According to his judgment, these vessels, or better said these *votive* figures, represented a disease special to Peru, an endemic variety of tuberculosis ("llaga" or "hutta=uta"). Mr. Espada knew only one note in the old literature which refers to mutilations of the lips and the nose. "The reyezuelos ó caracas of the Isle of Puna mutilated in this way their eunuchs, for the purpose of making them unattractive to the concubines." Zarate relates it (*Histoire de la decouverte et de la Conquete du Perou*, translated from the Spanish of Augustin de Zarate by S. D. C.; first Vol., Paris, by the Compagnée des Libraries, M.D.CC.XLII. with the privilege of the King, page 25): "Le Seigneur de cette isle (de Puna) était fort crainte et fort respecte par ses sujets, et si jaloux que tous ceux qui étoient commis à la garde de ses femmes, et même tous les domestiques de sa maison, étoient eunuques; et on coupoit non seulement les parties qui servent à la generation mais pour les defigurer on leur coupoit aussi le nez." Oviedo says that the lips also were sometimes amputated. Herrera mentions no mutilation. Nor do Rivero and Tschudi (*Antigüedades peruanas*, Vienna, 1851). Bastian (*Die Culturländer des Alten America*, Berlin, 1878, Tom. 1, p. 593) says the same as Oviedo, that "they also amputated the nose and lips, so that they would not present a seductive appearance."

Prof. Virchow formulated his judgment, saying that he neither believed that they treated of punished criminals, because it was not related in the literature. Besides there exists statues of wood[1] of prisoners, derived from the Isla Chincha (Guana isles); two are well preserved, one great and the other small. The great one is on foot, the little one is represented as a truncated body. On

[1] See Virchow, [illegible], 1873.)

both figures the arms are held arranged behind, like a person who listens tranquilly. The large idol has a cord round the neck, which is tied in front by a coarse knot. One of the ends of the cord goes down to the stomach. The nose in both takes the form of an eagle's beak. David Forbes says these wooden idols represent prisoners holding a cord or a serpent to the neck. Forbes and H. B. Frank suppose that they have thus symbolized syphilis, a disease original to the mountains of Peru and characteristic of the alpaca or llama, an animal which transmitted it to man by unnatural vice. Neither of these idols nor those described by Weiner represent mutilated nose and lips. Therefore *all* prisoners were not punished by amputation of nose and lips. (See rich collection in La Plata Museum.)

Polakowsky divides all these vessels into groups: 1. Clay figures representing mutilation of nose, of pathologic origin; 2. Those where it is doubtful whether they treat of disease or of surgical operation.

Polakowsky does not think they treat of punished criminals, because he has searched for data in the literature and failed to find such. He lived twenty-five years in South America. Von den Steinen found in the Royal Museum of Berlin representant vases of heads and entire bodies, one of them stretched on his belly, the other on the knees or with the legs crossed. All had mutilations of the point of the nose and the greater part of the upper lip. In four of the pieces the feet were lacking, on the others the lower part of the body was covered with a cloth which enveloped it from the hips, in a manner which made one think they also had lost the feet.

Now in ceramics too: First, we have types undoubtedly of prisoners, representing a person on foot with hands behind and bound with a cord, but no other indication to show that it treats of a prisoner. Secondly, a prisoner on his knees, halting, or sitting with the feet crossed. Moreover, he has a cord tied around his neck. A *third* represents the serpent eating a certain part of his body (penis), while his hands are tied behind his back. But in *none* of these clay figures which represent undoubtedly prisoners, was there mutilation of any part of the face or of the body. The testimony of the huacos potteries, therefore, is to the effect that the Old Incans did not mutilate their prisoners by amputation of the feet. Moreover, in

these ceramics whenever amputation of feet is represented (for the flaps are shown) there is evidence of disease in the face.

Does there exist such a disease of the face, which would also affect the feet to require amputation of them and both equally? Yes! I believe that the amputated feet of the huacos potteries have relation with the mutilations represented on the face.

Mr. Ambrosetti (*Nota de Arqueologia Calchaqui* Instituto Geographico-Argentina, tomo xvii) thinks that the stumps are due to the imperfect work of the artist, like in Calchaque idols, whose feet are are not moulded in form at all. But then there are images shown stretched on the belly, apparently intended to be shown in a helpless condition: I have seen one representing a person who was dressing his stump with a cup of medicine, the stump thrown across the opposite leg; and besides there are the flaps shown and also that foot specimen itself, like a foot that had been cut off. Some of these amputated figures are represented with the hand extended for alms; some hold a stick to creep or hobble with on their knees, with their feet cut off.

In the images of the La Plata Museum, shown among the ten which I print in this article, it can be seen by the originals (for all the kneeling figures are without feet, the ends of their limbs showing flap-stumps as if amputated, which cannot be seen by a front view) that in no case is amputation represented without the image showing a diseased face. Now the ancient Incans cut off the hands and ears of prisoners, but not the feet. Yet this mutilation *of hands and ears* is not shown by a single specimen of pottery that I have seen, and besides I believe that they never buried a *clay soul-figure* with such a criminal. They *wanted him to die*. The pot buried with him would keep him alive.

In a report of the Viceroy, Dr. Martin Henriquez, of the year 1582, which mentions the manner of government of Peru, the customs and usages of the Incas, and where it is said in a general way that amputation of limbs was a punishment of criminals, he goes on to say: "But in my opinion such amputations were no simple bodily punishment which left the sufferer alive, but a kind of capital execution like hanging, or other like." The text, which is here translated literally, says: "Executions were public and very crude. Some were precipitated from rocks (of Andean precipices), others had their limbs amputated, etc."

Von den Steinen says: "As to the mutilations of the legs, whether

it be amputation or disease we have no case made out. In all Peruvian vases where feet are represented they are easy to be recognized as such. The accuracy in the rendering goes even so far that in some representations of persons with tucked-under legs the form of the feet is expressed on the bottom of the vase. That the Old Peruvians liked to find in their vessels the forms of persons affected with remarkable manifestations of disease is shown also in the Berlin collection, by the large number of them blind, one-eyed, with lop-sided jaws, etc. As to the finding places of these vases, they are unfortunately not safely established, the greatest part has the indication of Chimbote, and besides there is Trujillo and Chancay."

I point out, in conclusion, here that the influence of cold of the Andean heights might have had to do with the necessity of amputation of feet. There was a great deal of barefoot walking in Incan climates, while the hands would be better clad. We must renounce, however, the giving of a positive judgment as to the mutilations of the feet of Old Peruvians. So far no other explanation has been found but a pathological one.

Prof. Bandelier wrote me from Lake Titicaca, where he was engaged in explorations for the American Museum: "All the Pachacamac remains, a few specimens perhaps excepted, which I cannot now remember, belong to the so-called Yunca (hot country) or coast Indian type of artifacts, and they are certainly anterior in date to 1532. I do not wish to be understood to say that all the Pachacamac finds to be made, or made previously, are not post-Columbian; but the site where I caused the excavations to be made and the depth at which the objects were taken out, point to the conclusion that my finds are indeed pre-Columbian, or at least with very few exceptions only. The human foot alone and in appearance amputated is not rare among coast pottery, and the Museum must have another one sent by me from Lambayeque, with its sandal perfectly normal as well as handsomely ornamented. I remember having seen other specimens of the same description. But none of them were deformed as the Pachacamac foot is.

"The deformed faces on the pottery are generally regarded as representations of syphilis, and I never heard leprosy mentioned in connection with them."

This is what I read of the ancient languages of Old Peruvians as written in their graves: There was never a migration of these dis-

eases from Asia, nor did their religious beliefs about the soul emanate from Asia. The surgery of ancient America was not of Asiatic derivation. The civilization or culture-growth of ancient Peruvians was purely an American institution which had developed from preëxisting savages on this hemisphere.

NEW YORK, 333 W. 23d St.

Stated Meeting, December 18, 1903.

President SMITH in the Chair.

The list of donations to the Library was laid on the table, and thanks were ordered for them.

The decease of the following members was announced:

Rev. Henry Clay Trumbull, D.D., at Philadelphia, on December 8, æt. 73.

Dr. Gustave Schlegel, at Leyden.

Mr. Rosengarten presented a communication on "The Earl of Crawford's MS. History in the Library of the American Philosophical Society."

Dr. Leonard Pearson was introduced by the President, and presented a paper on "The Animal Industries of the United States."

The President delivered his "Annual Address."

THE EARL OF CRAWFORD'S MS. HISTORY IN THE LIBRARY OF THE AMERICAN PHILOSOPHICAL SOCIETY.

BY JOSEPH G. ROSENGARTEN.

(*Read December 18, 1903.*)

In the Library of the American Philosophical Society there are four folio MS. volumes, in old binding with clasps, but with nothing on the Library records to show how they ever got there.

The 1st volume is labeled "Account of Some Campaigns of the British Army from 1689 to 1712, and Journal of a Campaign under Prince Eugene on the Upper Rhine, and Miscellaneous Papers."

The 2d and 3d volumes, "Journal of a Voyage from the Thames to Russia, and of Campaigning with the Russian Army, 1738–9."

The 4th volume, "Journal of a Campaign with the Russian Army against Turkey, 1739."

They are all in admirable condition, in uniform clerkly clear handwriting, and with a large number of fine maps, executed by Henry Köpp by order of the Earl of Crawford, and inscribed to the King of England, Lord Loudoun and other noted persons high in command in the British army.

The 1st volume contains:

1st. "A Short Treatise of Fortification and Geometry," pp. 33.

2d. "A Method of Discipline proposed for the Behaviour of a Regiment of Foot upon action," pp. 29.

3d. "An Account of the most remarkable Transactions which happened in the Campaigns I made from the Year 1689 to the Conclusion of the Peace of Ryswick in 1697." It begins as follows: "The Regiment I served in is very well known by the Title it bears of the Royal Regiment of Foot in Ireland, from which Regt. I may without Vanity say our British Infantry had the Groundwork of their present Discipline." It describes Schomberg's Irish campaign of 1689, pp. 12; then the campaigns of 1694–5–6–7, pp. 14.

Then, 4th, The Campaigns of 1702–12, pp. 63.

Then, 5th, comes "A Journal and Remarkable Observations during Three Campaigns made by a friend to the Trade of War, in Three Volumes. Vol. First: Journal of a Campaign made with the Imperial Army under the Command of Prince Eugene of Savoy on

the Upper Rhine in the Year 1735." It begins in London, May 24, 1735, pp. 87, and closes: "I shall conclude this Campaign with the inserting a few usefull Papers I collected during the Operations of it, and of the following (I dare venture to say) exact Plan of the Country we march'd over." Then follow twelve well-executed maps or plans giving the successive positions of the armies, the last an elephant folio map of the Rhine from Coblenz to Carlsruhe.

6th. "The following March Root [*sic*] ordered by Prince Eugene . . . . I insert as a Model . . . . of a very difficult part of Duty in the Trade of War," pp. 5.

7th. "Un detail exact et bien calculée [*sic*] de ce que coutoit par mois en 1681 la plus florissante marine que la France aie ene," pp. 8.

8th. "Un Traittes Touchant Les Conquêtes qu'on pourroit faire en Amerique sur la Maison de Bourbon au cas que la Guerre devienne generale et qui seuls peuvent retablir l'Equilibre de l'Europe," pp. 6.

9th. "Tabulated Lists of the French Army in 1735."

10th. "Reflexions sur les Evenemens de la Mosellé," pp. 2 [imperfect].

11th. "Treaty and Cartell made and Concluded between His Imperial and Catholick Majesty on the one part, and his most Christian Majesty on the other, concerning their Prisoners of War in their Armies on the Rhine, 1735," pp. 16.

12th. "The Troops in the Black Forest, Friburg and Brisac—Specifications of the Imperial Regiments 1735. A List of the Imperial Regiments both Horse and Foot, the Names of the Comissioned Officers, Comanders and Agents, together with the Numbers of men in each Compleat Regt., and the places where they are now," pp. 24. On the fly leaves are a tailor's bill in German and some heads of letters, etc.

The 2d and 3d volumes are labeled "Journal of a Voyage from the Thames to Russia and of Campaigning with the Russian Army." It begins at Gravesend, April 13, 1738, and contains personal expenses, phrases in English, German and Russian, maps, drawings of scenes, camps, etc., and a sermon on Peter the Great. The 2d vol. pp. 287, the 3d vol. pp. 398. There are eighteen maps of sieges, operations, defences of Belgrade, etc., etc. One of the maps is dedicated to George the Second.

The 4th volume opens with

1st. "A Tabular List of the Imperial Troops in 1737," followed by (in French)

2d. "Journal of the Hungarian Campaign of 1737," with General Orders, Maps, etc.

3d. "Relation des Operations de la Campagne 1738," by Chevalier de Forrestier, Captain of the Regiment of the King's Infantry.

4th. "A Diary of the Army under the Duke of Lothringen, 1738–9" (in German).

The bills for personal expenses are made out to the Earl of Crawford, and this is the only identification. These volumes were no doubt prepared by his secretary, under his direction, as material for establishing the record of his services for preservation in the family archives, and for use in a posthumous biography.

The author and owner of these MS. volumes was John Lindsay, twentieth Earl of Crawford. The *National Dictionary of Biography* gives, in Vol. 38, p. 305, etc., the following sketch of his life: 1702–1749. "After attending the Universities of Glasgow and Edinburgh, he was sent in 1721 to the Military Academy of Vaudreuil, Paris. In 1726 he was appointed to a company in one of the additional troops of the Scotch Greys. He early acquired a reputation for resolution and daring, and while not neglecting intellectual accomplishments, attained exceptional proficiency in athletic exercises, especially in shooting, fencing, riding and dancing. On the disbandment of the additional troops of Scots Greys in 1730, he . . . . devoted his more serious attention to military studies and his leisure to boating and hunting. On January 3, 1732, he obtained command of a troop of the 7th Queen's own regiment of dragoons, ... in February, 1734, he obtained a captain lieutenancy in the 1st regiment of foot-guards, and in October a captaincy in the 3d regiment of foot-guards, but being desirous of acquiring practical acquaintance with the art of war, he got permission, in 1735, to join the Imperial army under Prince Eugene. He specially distinguished himself at the battle of Claussen on October 17. In April, 1738, he sailed from Gravesend to St. Petersburg, and having received from the Czarina Anna the command of a regiment of horse, with the rank of general, he, after a perilous journey of one thousand miles, joined the army of Marshal Munich, then engaged in a war against the Turks. He soon acquired great proficiency in the mode of warfare practiced by the Russians. . . . . After the retreat of Munich to Kiow, Crawford left him and joined the Imperialists near Belgrade. When the army went into winter

quarters, he accompanied Prince Eugene's regiment to Comorn, and thence proceeded to Vienna, still occupying his leisure principally in military studies. In April he rejoined the Imperialists at Peterwardein under Marshal Wallis. . . . . He left in August, 1741, and returned to England. In July, 1731, he had been made colonel of horse and adjutant-general; in October, Colonel of the 42d Highlanders, and in December, Colonel of the Grenadier Guards. . . . . In May, 1743, he joined the army under the Earl of Stair at Hochstet, when he was made colonel of the Scotch troop of horse guard and adjutant-general. At the battle of Dettingen, on June 16, he commanded the brigade of life-guards, ... with the rank of brigadier-general. He joined the allied army near Brussels in the following May, and at the battle of Fontenoy, April 30, 1745, he succeeded . . . . in so covering the retreat that it was effected in perfect order. On 30th May following he was made a major-general. On the outbreak of the rebellion in Scotland in 1745, he was appointed by the government to the command of six thousand Hessians, with whom he secured the towns of Perth and Sterling and the passes into the lowlands. . . . . He rejoined the army in the Netherlands. On the day of the battle of Roncroux, October 5, 1746, he was surprised while reconnoitring, but coolly assuming the character of a French general, ... was permitted to pass unmolested. . . . . In December he was appointed to the command of the 25th foot . . . . on May 20, 1747, to that of the Scots Greys, and on September 20 was made a lieutentant-general. . . . . Crawford joined the Duke of Cumberland in the campaign of 1748. . . . . Returning after the peace to London, he died there September 20, 1749."

The *Dictionary of National Biography* refers to "Memoirs of the Life of the Right Hon. John Lindsay, Earl of Crawford and Lindsay, by Richard H[should be R]olt, London, 1753, 4to," reprinted in 1769, under the title "Memoirs of the Life of the late Rt. Hon. John Earl of Crawford, describing many of the highest achievements of the late wars," and "Lord Lindsay's Lives of the Lindsays, London, 1849, 3 volumes, 8vo." In Vol. 2, p. 235, etc., there is a sketch of his life, and on p. 237 a note says: "The diary of this journal [*i. e.*, from Petersburg to General Munich's quarters], dictated by Lord Crawford and corrected by his own hand, a large folio, is now in my possession, with various other journals and military MS., the bequest of my kind relative, Lady

Mary Lindsay Crawford, sister of the last Earl of Crawford of the Byres line."

The full title of Rolt's book is "Memoirs of the Life of the late Right Honorable John Lindesay, Earl of Craufurd and Lindesay, Lord Lindesay of Glenesk and Lord Lindesay of the Byers, one of the sixteen Peers of Scotland, Lieutenant General of his Majesty's Forces, and Colonel of the Royal North British Grey Dragoons, by Richard Rolt, author of The True History of the Late War. London, Printed for Henry Köpp; and sold by Mr. Newberry, in St. Paul's Church Yard; Mr. Owen, at Temple Bar; and Mr. Paterson, in the Strand. 1753, 4to, pp. 432, with appendix."

Much of this book is a reprint of the principal parts of the four MSS. volumes in the Library of the American Philosophical Society, viz.:

Chapter 2. An account of the rise of the war between the Emperor and France in 1733 to the campaign on the Rhine in 1735, when the Earl of Craufurd served as a volunteer under Prince Eugene and Count Seckendorff; the action at Claussen; and the end of the war.

Chapter 3. The rise of the war between the Russians and the Turks in 1736, wherein the Imperialists were auxiliary to the former; the state of those empires, with a short account of the campaigns in Tartary and Hungary in the years 1736–7.

Ghapter 4. An account of the Earl of Craufurd's preparations for the Russian campaign of 1738; his voyage to St. Petersburg; his reception at that Court, and his journey from thence to the Russian army in Bessarabia. His reception by Feldt-marshal Munich; an account of the Tartars; as also of the campaign in Turkey, and the Earl of Craufurd's journey to the Imperial army in Hungary. His reception by the Grand Duke of Tuscany, an account of the campaign in Hungary, and his lordship's journey to Vienna.

Book III, Chap. 1. The campaigns of 1739, containing the journal of the campaign in Hungary, together with a general plan of the whole, which was generally printed for this work by his Royal Highness Prince Charles of Lorraine; as also an account of the same campaign written by the Earl of Craufurd, with a description of the battles of Krotza and Panscora; to which is added a short detail of the Russian campaign, with his lordship's observa-

ment of value to students of military history. The Philosophical Society would no doubt be glad to exchange them for books better suited to its peaceful scientific pursuits and to its legend, prescribed by its great founder, Franklin, " For promoting useful knowledge."

EXTRACT FROM THE PRESIDENT'S ADDRESS.

BY EDGAR F. SMITH.

(Read December 18, 1903.)

Amid the activities of the year, while we all have been busy in our several vocations, word has been passed from time to time that the grim Messenger had appeared in our circle and summoned to the silent majority not a few of our fellow-members. This roll included—

DR. CHARLES SCHAFFER, who was born in this city sixty-six years ago. He graduated from the Medical Department of the University of Pennsylvania at the age of twenty-one. His poor health prevented him from devoting himself wholly to his chosen profession. He was deeply interested in the natural sciences and their applications. He took an active part in the affairs of the Academy of Natural Sciences, the Historical Society of Pennsylvania, the Franklin Institute, the Photographic Society of Philadelphia, the American Association of Mining Engineers, and was particularly well known in the scientific world for his work in the field of botany. His knowledge of the local species of plants and shrubs was profound. He made a special study of the mountain flora of British Columbia. He died November 23, 1903.

DR. THOMAS G. MORTON was born in this city on August 8, 1835. After preparation in the city schools and the College of the University of Pennsylvania he entered the Medical Department, from which he graduated in 1856.

He devoted himself chiefly to general surgery and was a resident physician in the Pennsylvania Hospital for some time. He was one of the founders of the Polyclinic Hospital and the Orthopedic Hospital. His many services to the State on various boards and commissions included the erection of the State Hospital for the Insane for the Southern District of Pennsylvania.

In 1880 he was chosen President of the Pennsylvania Society for the Restriction of Vivisection and Vice-President of the Society to Protect Children from Cruelty. Dr. Morton had great success as a surgeon. His clinical lectures at the Pennsylvania Hospital were attended by thousands of students from all parts of the world. He was a member of various foreign and American professional bodies,

and a frequent contributor to journals of medicine. His own books are *The Transfusion of Blood* and *The Surgery of the Pennsylvania Hospital*, together with a history of the latter institution. He was actively engaged in public school work in this city, and for many years was a member of the Board's most important committees. He was a member of the Academy of Natural Sciences and of the Union League. He died May 20, 1903, at Cape May.

Rev. Henry Clay Trumbull was born in Connecticut, June 8, 1830. He received his education in Williston Seminary.

In 1858 he became a missionary for the State Sunday School Association, which had its headquarters in Hartford. He was a chaplain in the Tenth Regiment in the Civil War. In 1863 he was taken prisoner before Fort Wagner and sent to the Charleston jail, later to Libbey Prison, where he was held for several months.

Five of his books treat of his army experiences. They were *Some Army Soldiers*, *The Knightly Soldier*, *A Biography of Major Henry Ward Camp*, *The Captured Scout of the Army of the James* and *War Memoirs of an Army Chaplain*.

At the end of the war he returned to Sunday School work. In 1866 he received the degree of M.A. from Yale, and in 1881 the degree of D.D. from Lafayette College, and the same degree from the University of New York in 1882. In 1875 he took charge of the *Sunday School Times* of this city, and during his editorial career wrote a number of books of a devotional character. He traveled in Egypt, Arabia and Syria, where he studied the track of the Exodus and identified the site of Kadash-Barnia. Following this sojourn abroad he prepared a number of volumes, some of which bear these titles, *The Ten Commandments as a Covenant of Love*, *Light on the Story of Jonah*, *Subjects in Oriental Social Life*, *The Threshold Covenant*, *The Covenant of Saul*, etc., etc. He died in this city on December 8th.

Prof. Robert H. Thurston was born in Providence, R. I. In 1859 he graduated from Brown University with the degree of Civil Engineer. He served throughout the Civil War, beginning in 1861, being finally promoted to the position of chief engineer of one of the monitors. For six years he was an instructor in the United States Naval Academy at Annapolis. In 1871 he became Professor of Mechanical Engineering in the Stevens Institute of Technology.

This position he held until 1885, when he was called to the directorship of the Sibley College of Mechanic Arts at Cornell University. His work in both of these institutions of learning was most successful. It was characterized by great energy and executive ability. In 1873 Dr. Thurston was United States Commissioner to the Vienna Exposition. In 1875 he was appointed a member of the United States Board to Test Metals. In connection with this work he devised a machine for torsional tests, and made numerous investigations in the Mechanics of Materials.

In 1883 he published a work in three volumes, bearing the title *The Materials of Engineering*. Other books by him, such as the *Handbook of Engines and Boiler Trials, Stationary Steam Engines and Boiler Explosions*, have had a wide circulation. His *Manual of the Steam Engine*, in three volumes, was translated into French. Some of his other publications are *Friction and Lubrication, Friction and Lost Work, The Animal as a Machine and Prime Motor*, and *The Life of Robert Fulton.*

His contributions to scientific and engineering periodicals ran up into the hundreds.

He was a member of many scientific societies at home and abroad. He was one of the founders of the American Society of Mechanical Engineers, and its first President. He was a Vice-President of the American Association for the Advancement of Science in 1877, 1878 and in 1884.

The scientific and engineering work of Professor Thurston was of great benefit to mankind, for he made engineers better scientists, promoted engineering education, helped to put engineering upon a higher plane, and was constantly watching to dispel the fogs of prejudice by help of the truths of science.

He was the recipient of the honorary degree of LL.D. from his alma mater, and of the degree of Doctor of Engineering from Stevens Institute.

"In all his relations to general University problems he exhibited the spirit of the scholar and the wisdom of the man of affairs. Serene in temper, sound in judgment, swift and certain in action, he justly exercised a weighty influence.

"As a colleague he exhibited an interest in all good learning and bespoke a good scholar and a general fellow-worker. As a friend and companion, he manifested a cordial sympathy that attracted all who knew him and held them in the bonds of an increasing affec-

tion. In all the relations of life he moved upon the highest levels and showed forth the better qualities of our nature."

His loss falls heavily upon all—his colleagues, his friends and his University—but most heavily upon his family, with whom we deeply sympathize.

WILLIAM VINCENT MCKEAN was born in Philadelphia, October 15, 1820, and died March 23, 1903. He was associate editor of the *Pennsylvania* with John W. Forney in 1852; chief clerk of the House of Representatives from 1853 to 1855; examiner of the United States Patent Office; private secretary to James Buchanan; an editorial writer for the *Philadelphia Inquirer* and *Public Ledger*. He edited the *National Almanac Record* for 1864, and wrote a report favoring the money order system for the United States in 1858, *What the Navy Has Done During the War* in 1864, *General McClellan's Campaign* in 1864, and delivered an address in Independence Hall on July 2, 1876, entitled "The Centennial of American Independence."

THEODORE D. RAND was born in Philadelphia, September 16, 1836, and graduated from the Episcopal Academy and the Polytechnic College. In 1858 he was admitted to the Bar and practiced Law for a time. He was chiefly known for his scientific work in connection with Mineralogy and Geology, having published a number of papers on these branches and lectured very frequently before scientific bodies. He was a member of the Mineralogical section of the Academy of Natural Sciences, also the Franklin Institute and the American Institute of Mining Engineers. He died on April 24, aged sixty-seven years.

EDWARD RHOADS. It was only last spring that this young scientist was received into our membership. No one at that time dreamed that he would not be with us now in the full vigor of manhood. His history is briefly as follows:

Dr. Rhoads graduated with honors from Haverford College in 1893. He studied from 1896–1898 at Johns Hopkins University, from which institution he received the degree of Doctor of Philosophy. Immediately thereafter he became instructor in physics in the Worcester Polytechnic Institute. Leaving here in 1901 he was

appointed to a similar position in Haverford College. Among his publications we find the following titles:

"The Effect of the Fibrous Structure of Sheet Iron on the Changes in Length Accompanying Magnetizations."

"Experiments on the Change in Dimensions Caused by Magnetization in Iron."

"Relations Between the Changes in Thermo-Electric Power Caused in Magnetization."

Dr. Rhoads was unmarried and resided with his mother in Germantown. He was a member of the American Association for the Advancement of Science.

CHARLES GODFREY LELAND was born in Philadelphia in 1824, and received his education at Princeton and the Universities of Heidelberg, Munich and Paris. He was very active in the Revolution of 1848 and was one of the American delegates to congratulate the Provisional Government.

He studied and practiced Law in Philadelphia for four years, beginning with 1849, and then devoted himself to Journalism and the writing of books. In 1869 he removed to Europe, living chiefly in London, and was occupied with literature. On his return to America in 1880 he devoted much time in introducing the minor arts as a branch of instruction in public schools. Since 1886 he has been residing in Florence. He has been a frequent contributor to the Oriental, Social Science and Folklore Societies.

He published *Poetry and Mystery of Dreams* in 1850, *Hans Breitman's Ballads* in 1858, *English Gypsies* in 1852, *English Gypsy Ballads* in 1873, *Life of Abraham Lincoln* in 1881, *The Minor Arts* in 1881, *Gypsy Sorcery* in 1891, *Etruscan Roman Forms* in 1892, and numerous other books. His specialty seems to have been the study of tradition and folklore. He passed away on March 20, 1903, in Florence, Italy.

JAMES GLAISHER, F.R.S., an honored foreign member, attained the grand age of ninety-four years. When but twenty years old he was made an assistant on the principal triangulation of the Ordnance Survey of Ireland. His chief work during his life was the investigation of subjects on practical Meteorology. His contributions in this field and in Astronomy are exceedingly numerous and valuable. His hygrometrical tables, first published in 1847, passed through

eight editions, and with his *Travels in the Air*, *Diurnal Range Tables*, *Report on the Meteorology of India* and *Meteorology of Palestine*, are among his chief writings. In the interests of meteorology he made twenty-nine balloon ascensions in four years. In the one of September 5, 1862, he and his companion attained the highest distance from the earth (37,000 feet) ever reached. He was a pioneer in the systematic organization of meteorological observations. In 1850 he was one of the founders of the Royal Meteorological Society, being its original Secretary, "who nursed it through its infancy and youth, and left it to other hands only when it was old enough and strong enough to walk alone." He passed away February 7, 1903.

PROF. WILLIAM HARKNESS was born in Scotland on December 17, 1837. He died at Jersey City, N. J., U. S. A., February 26, 1903.

From *Science* we learn that he graduated in 1858 as an A. B. from Syracuse University, from which institution he also received the degrees of A. M. (1861) and LL. D. (1874). In 1862 he received the degree of M. D. from a New York school, and in August of that year became aid to the U. S. Naval Observatory. In August, 1863, he was commissioned Professor of Mathematics in the Navy with the rank of lieutenant-commander. From October, 1865, to June, 1866, he served on the U. S. Monitor Monadnock, making observations on the behavior of her compasses under the influence of the heavy iron armor of the ship. This was the most elaborate discussion of the behavior of compasses on armed ships that has ever been made. His report was published by the Smithsonian Institution in a volume of 125 quarto pages. On his return to Washington he was attached to the Hydrographic Office for one year, and thereafter for seven years to the Naval Observatory, during which period he observed the total solar eclipse at Des Moines, Iowa, and discovered the famous coronal line λ 1474, also the total solar eclipse of December 22, 1870, at Syracuse, Sicily, and in 1871 was appointed one of the original members of the U. S. Transit of Venus Commission to arrange for the observation of the transits of that planet in 1874 and in 1882. He devised most of the instruments for the purpose and fitted out the various expeditions in this country. His own station was at Hobart, Tasmania.

In 1875 he studied the observations of the U. S. parties from a series of 100 collodion photographs on glass plates. He car-

ceeded where others failed, and in the course of his study in 1877 invented the spherometer caliper. In 1879 he discovered the theory of the focal curve of achromatic telescopes. In 1876 he set up the Government astronomical exhibit at the Centennial Exposition in Philadelphia, Pa. In 1878 he observed the transit of Mercury at Austin, Texas, and the total solar eclipse at Creston, Wyoming, in July of that year, 1878. He carried on extensive experiments in astronomical photography, and in 1881 to 1883 was engaged in reducing the zones of stars observed by Capt. Gillin at Santiago, Chile, in 1849 to 1852. In 1888–9 and 1890 he gave much time to the preparation of his work on *The Solar Parallax and Its Related Constants*. From 1891-1899 he was chiefly occupied in following the erection of the new Naval Observatory, in devising and mounting its instruments, etc., etc. In 1894 he became astronomical director of the Naval Observatory, with complete control of all its astronomical work. He also became director of the *Nautical Almanac* in June, 1897. These offices he held until his retirement for age, December 17, 1899, with the rank of rear-admiral. He was the author of many scientific papers and member of numerous scientific societies, President of the Washington Philosophical Society in 1887, and President of the American Association for the Advancement of Science in 1893.

Prof. J. Peter Lesley. There are those in this audience who can speak more fully of this departed friend than the speaker. These halls knew the great geologist well. The interests of this Society were his. Many hours did he bestow upon its affairs, and about us there are many evidences of his unselfish labors. My knowledge of him was very slight. I saw him fréquently in the halls and the museum of the University of Pennsylvania, but beyond the formal bow it was not my privilege to know him. In the September issue of the *American Geologist* for 1903 our associate, Dr. Persifor Frazer, has recorded a picture of this successful teacher and investigator, from which we abstract the following facts. This city was Lesley's birthplace. The natal day was September 17, 1819. His training was received here and in the University, where he completed his studies in 1838. At Princeton he studied Theology from 1841 to 1843, and in 1844 obtained his ministerial license. The year of 1844-45 he spent in study at the University of Heidelberg. For five years (1846-1851) he officiated as pastor of the

Congregational Church in Milton, Mass. At the expiration of that period he left the ministry and devoted his whole time to geological pursuits. In 1872 he became Professor of Geology and Mining in the University of Pennsylvania, as well as the Dean of its Faculty of Science. In 1874 he was entrusted with the directorship of the second geological survey of this State.

"The hundred volumes and thousands of maps and sections of this survey will be his most enduring monument."

He threw great light upon the rock oil problem. He was one of the original members of the National Academy of Sciences; President of the American Association for the Advancement of Science in 1884, and author of *Man, His Origin and Destiny, from the Platform of the Sciences; Coal and Its Topography, etc.*

"Lesley's character was wholly noble. He was generous to prodigality towards others while careless of his own ease and comfort. Plain living and high thinking was the motto which moulded his life."

Surrounded in his closing years by a loving wife and devoted daughters, he peacefully passed away on June 1, 1903, at Milton, Mass.

Sir George Gabriel Stokes was born August 13, 1819, at Skreen, County Sligo, of which parish his father was rector. He entered Pembroke College, Cambridge, in 1837, graduated in 1841, became Fellow of the College in the same year, and in 1849 was elected Lucasian Professor of Mathematics.

Professor Tate writes, "To us, who were mere undergraduates when he was elected to the Lucasian Professorship, but who had with mysterious awe speculated on the relative merits of the man of European fame whom we expected to find competing for so high an honor, the election of a young and (to *us*) an unknown candidate was a very startling phenomenon, but we were still more startled a few months afterwards when the new Professor gave public notice that he considered it part of the duties of his office to assist any member of the University in difficulties he might encounter in his mathematical subjects. Here was, we thought, a single knight fighting against the whole melee of the tournament, but we soon discovered our mistake, and felt that the undertaking was the effect of an earnest sense of duty on the conscience of a singularly modest but exceptionally able and learned man, and so it has

proved." Stokes may justly be looked upon as in a sense one of the intellectual parents of the school of Natural Philosophy which Cambridge has nurtured, the school which numbers in its ranks Sir William Thompson and Sir William Maxwell.

He was really a great discoverer in Mathematics and Physics. Stokes fully apprehended the physical basis of spectral analysis and pointed out how it could be applied to the detection of the constituents of the atmosphere of the sun and stars. In some of his earlier papers he has laid down the scientific distinction between rotational and differentially irrotational motion, which forms the basis of Helmholtz's magnificent investigations about vortex motion. His papers on the (long) spectrum of the electric light, and particularly those on the absorbent spectrum of the blood, are of very great value.

Sir William Thompson says that Stokes roamed over the whole domain of Natural Philosophy in his work and thought, Electricity being the single field which he looked upon from the outside. He even enriched pure Mathematics of a highly transcendental kind. Mathematics with Stokes was the servant and assistant, not the master. In science his guiding star was Natural Philosophy. In 1843 he published his *Theory of the Viscosity of Fluids* and a little later his *Theory of Oscillatory Waves*. "The Dynamical Theory of Diffraction" was one of his most important contributions on the subject of light. In his paper on "The Change of Refrangibility of Light" he described his now well-known discovery of Fluorescence, according to which a fluorescent substance emits in all directions from the course through it of a beam of homogeneous light.

Stokes' scientific work and scientific thought are but partially represented by his public writings. He gave generously and freely of his treasures to all who were fortunate enough to have the opportunity of receiving from him.

Sir William Thompson says "that his teaching me the principles of solar and stellar Astronomy while we were walking about among the Colleges, sometime prior to 1852, is but one example of his generosity."

The funeral of this great man took place last February at Cambridge, England. The most distinguished representatives of many branches of learning were present. The University church was crowded in every part. The assembly constituted a living witness

of the esteem in which the memory of Sir George Stokes is held in the intellectual world. The coffin containing the late Master's body was first carried around the court of Pembroke College, in accordance with an ancient custom reserved for Masters, the procession being formed of the choir and officiating clergy, the Fellows of the College, former Fellows, Masters of Arts, Bachelors of Arts and undergraduates. The interment took place at Mill Road Cemetery.

In the words of Lord Kelvin, "The world is poorer through his death, and we who knew him feel the sorrow of bereavement."

Prof. Josiah Willard Gibbs. From the *American Journal of Science* for September, 1903, we glean that he was born February 11, 1839, in New Haven, where his father was professor of Sacred Literature in the Yale Divinity School. He entered Yale College in 1854 and graduated in 1858. During his academic course he received several prizes in Latin and Mathematics. In 1863 he won the Ph.D. degree and was appointed to a tutorship in the College. The winter of 1866-67 he spent in Paris, and the year following went to Berlin, where he heard Magnus and others in physics and mathematics. In 1868 he listened to Kirchhoff and Helmholtz at Heidelberg. In 1871 he became Professor of Mathematical Physics at Yale; this position he held until the time of his death. It was not until he was thirty-four years old that he gave to the world, by publication, evidence of his extraordinary powers as an investigator in Mathematical Physics. In 1876 and 1878 appeared two parts of his great paper "On the Equilibrium of Heterogeneous Substances." The third overshadowed these somewhat. This is his most important contribution to physical science. It is one of the greatest and most enduring monuments of the wonderful scientific activity of the nineteenth century.

The publication of this work was universally regarded an event of the first importance in the history of Chemistry. It founded a new department of chemical science. Yet years elapsed before its value was generally recognized. It was translated into German in 1891 by Ostwald, and into French in 1899 by LeChatelier.

In 1881 and 1884 he printed for private use a concise account of vector analysis. This he applied to some of the problems of astronomy. In 1888 to 1889 he contributed five papers on points in the electro-magnetic theory of light and its relations to the various elas-

tic theories. His last work was upon *Elementary Principles in Statistical Mechanics.*

The value of Williard Gibbs' work to science has been formally recognized by many learned societies and universities at home and abroad. He was a member of the National Academy of Sciences, the Royal Institute of Great Britain, the Royal Society of London, etc., etc., and the recipient of honorary degrees from Williams College and from the Universities of Erlangen, Princeton and Christiana. In 1881 he received the Rumford medal from the American Academy of Boston, and in 1901 the Copley medal of the Royal Society of London.

His life was uneventful. He made but one visit to Europe. He lived in New Haven, in the same home which his father built, a few rods from the school where he prepared for College and from the University, in the service of which his life was spent. He never married. He was retiring in disposition, went little into society and was known to few outside the University. His modesty in regard to his work was proverbial. "Unassuming in manner, genial and kindly in his intercourse with his fellow-men, devoid of personal ambition of the baser sort, or of the slightest desire to exalt himself, he went far toward realizing the ideal of the unselfish Christian gentleman." He died April 28, 1903.

They are gone. The world and we shall miss them. May the good they have accomplished serve as further incentives to us to press forward—each in his own specialty—without ceasing, in quest of the all-satisfying, all-enlightening truth.

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LIST OF MEMBERS

OF THE

American Philosophical Society

HELD AT PHILADELPHIA

FOR PROMOTING USEFUL KNOWLEDGE

(Founded 1743)

January, 1904.

LIST OF MEMBERS

OF THE

AMERICAN PHILOSOPHICAL SOCIETY

JANUARY, 1904.

A

	Name.	*Date of Election.*	*Present Address.*
1687.	ABBE, CLEVELAND, Prof.	July 27, 1871,	U. S. Weather Bureau, Washington, D. C.
1463.	ABBOT, HENRY L., Gen. U.S.A.	April 18, 1862,	23 Berkeley St., Cambridge, Mass.
2311.	ABBOTT, ALEXANDER C., M.D.	Feb'y 19, 1897,	University of Pennsylvania, Philadelphia.
2170.	ABBOTT, CHARLES CONRAD, M.D.	Dec. 20, 1889,	Trenton, N. J.
1809.	ÅCKERMAN, RICHARD, Prof.	July 21, 1876,	Stockholm, Sweden.
2128.	ADAM, LUCIEN.	Dec. 17, 1886,	41 Bard Sevigné, Rennes, France.
2457.	ADAMS, CHARLES FRANCIS, LL.D.	Feb. 15, 1901,	23 Court St., Boston.
2451.	ADLER, CYRUS, Ph.D.	May 18, 1900,	Smithsonian Institution, Washington, D. C.
1779.	AGASSIZ, ALEXANDER, Prof.	April 16, 1875,	Cambridge, Mass.
1642.	AGASSIZ, MRS. ELIZABETH	Oct. 15, 1869,	Quincy St., Cambridge, Mass.
2380.	ALLEN, ALFRED H., F.C.S.	May 20, 1898,	67 Surrey St., Sheffield, Eng.
1869.	ALLEN, JOEL ASAPH, Prof.	Sept. 20, 1878,	Am. Museum of Natural History, New York.
1927.	AMES, REV. CHARLES G.	Jan'y 21, 1881,	12 Chestnut St., Boston, Mass.
2064.	ANDERSON, GEO. L., Maj. U.S.A.	Feb'y 19, 1886,	Ordnance Board, Governor's Island, New York City.
2164.	ANGELL, JAMES B., LL.D., Pres't.	Oct. 18, 1889,	Ann Arbor, Mich.
2220.	APPLETON, WILLIAM HYDE, Prof.	May 19, 1893,	Swarthmore, Pa.
2012.	ASHHURST, RICHARD L.	April 18, 1884,	319 S. 11th St., Philadelphia.
2019.	AVEBURY, The Right Hon. Lord.	July 18, 1884,	High Elms, Down, Kent, Eng.

B

1995.	BACHE, R. MEADE.	Jan'y 18, 1884,	4400 Sansom St., Philadelphia.
1832.	BACHE, THOMAS HEWSON, M.D.	Feb'y 2, 1877,	233 S. 13th St., Philadelphia.
2389.	BAER, GEORGE F.	Dec. 16, 1898,	1718 Spruce St., Philadelphia.
2295.	BAILEY, L. H., Prof.	May 15, 1896,	Cornell University, Ithaca, N.Y.
1630.	BAIRD, HENRY CAREY	Jan'y 15, 1869,	810 Walnut St., Philadelphia.
1991.	BAIRD, HENRY M., Prof.	Jan'y 18, 1884,	219 Palisade Ave., Yonkers, N.Y.
2419.	BALCH, EDWIN SWIFT.	Dec. 15, 1899,	1412 Spruce St., Philadelphia.
2467.	BALCH, THOMAS WILLING	May 17, 1901,	1412 Spruce St., Philadelphia.
2345.	BALDWIN, JAMES MARK, Prof.	Oct. 15, 1897,	Princeton, N. J.
2191.	BALL, SIR ROBERT STAWELL	May 15, 1891,	Observatory, Cambridge, Eng.
1965.	DEBAR, HON. EDOUARD SÈVE	July 21, 1882,	Ramsgate, England.
1741.	BARKER, GEORGE F., LL.D., Prof.	April 18, 1873,	3909 Locust St., Philadelphia.
2011.	BARKER, WHARTON	April 18, 1884,	119 S. 4th St., Philadelphia.
2489.	BARNARD, EDWARD E., Sc.D	April 8, 1903,	Yerkes Observatory, Williams Bay, Wisconsin.

	Name.	*Date of Election.*	*Present Address.*
1902.	BARTHOLOW, ROBERTS, M.D. . .	April 16, 1880,	1525 Locust St., Philadelphia.
2490.	BARUS, CARL, Ph.D., Prof. . . .	April 3, 1903,	30 Elm Grove Ave., Providence, Rhode Island.
2119.	BASTIAN, ADOLPH, Prof.	Dec. 17, 1886,	Königgrätzerstrasse 120, Berlin, Germany.
2421.	BAUGH, DANIEL	Dec. 15, 1899,	1601 Locust St., Philadelphia.
2482.	BECQUEREL, ANTOINE-HENRI, Prof.	April 4, 1902,	6me d'Urville, Paris, France.
1968.	BELL, ALEXANDER GRAHAM, Prof.	July 21, 1882,	1331 Connecticut Ave., Washington, D. C.
1802.	BELL, SIR LOWTHIAN, Bart. . . .	April 21, 1876,	Northallerton, England.
2255.	BEMENT, CLARENCE S.	May 17, 1895,	3907 Spruce St., Philadelphia.
2326.	DEBENNEVILLE, JAMES S	Oct. 15, 1897,	University Club, Philadelphia.
2264.	BERTHELOT, MARCELIN P. E., D. ès-Sc.	May 17, 1895,	Palais de l'Institut de France, Rue Mazarin, No. 3, VIe., Paris, France.
2253.	BERTIN, GEORGES	May 17, 1895,	11 bis Rue Ballu, Paris.
1920.	BIDDLE, CADWALADER	Oct. 15, 1880,	1420 Walnut St., Philadelphia.
1831.	BIDDLE, HON. CRAIG	Feb'y 2, 1877,	2033 Pine Street, Philadelphia.
2134.	BILLINGS, JOHN S., M.D.	Feb'y 18, 1887,	40 Lafayette Place, New York.
2256.	BISPHAM, GEORGE TUCKER . . .	May 17, 1895,	1805 DeLancey Place, Phila.
2157.	BLAIR, ANDREW A.	May 17, 1889,	406 Locust Street, Philadelphia.
1669.	BLAKE, WM. PHIPPS, Prof. . . .	Oct. 21, 1870,	Tucson, Arizona.
2491.	BOAS, FRANZ, Ph.D.	April 3, 1903,	Am. Museum of Nat. History, Central Park, New York.
1444.	VON BÖHTLINGK, M. OTTO, . . .	Jan'y 17, 1862,	Seeburgstrasse 35, II, Leipzig, Germany.
2235.	BONAPARTE, PRINCE ROLAND . .	Feb'y 15, 1895,	10 Ave. d'Jena 22, Paris, France.
1126.	BOYÉ, MARTIN H., Prof.	Jan'y 17, 1840,	Coopersburg, Lehigh Co., Pa.
1826.	BRACKETT, CYRUS FOGG, Prof. . .	Feb'y 2, 1877,	Princeton, N. J.
2083.	BRANNER, JOHN C., Prof.	May 21, 1886,	Stanford University, Cal.
2476.	BRASHEAR, JOHN A., Sc.D. . . .	April 4, 1902,	1954 Perryville Ave., Allegheny, Pa.
2095.	BREZINA, ARISTIDES, Dr.	May 21, 1886,	XIIIe St. Veitgasse, 15. Vienna, Austria.
2069.	BRINTON, JOHN H., M.D	Feb'y 19, 1886,	1423 Spruce St., Philadelphia.
2433.	BROCK, ROBERT C. H.	Dec. 15, 1899,	1612 Walnut St., Philadelphia.
2445.	BROEGGER, W. C., Prof.	Dec. 15. 1899,	Christiania, Norway.
2080.	BROOKS, WILLIAM KEITH, Prof. .	May 21, 1886,	Johns Hopkins Univ., Baltimore, Maryland.
2466.	BROWN, AMOS P., Prof.	May 17, 1901,	20 E. Penn St., Germantown. Philadelphia.
1881.	BROWN, ARTHUR ERWIN	April 18, 1879,	1208 Locust St., Philadelphia.
2394.	BROWN, ERNEST WILLIAM, Prof.	Dec. 16, 1898,	Haverford College, Pa.
2275.	BRUBAKER, ALBERT P., M.D. . .	Oct. 18, 1895,	105 N. 34th St., Philadelphia.
1547.	BRUSH, GEORGE J., Prof.	Jan'y 20, 1865,	Yale Univ., New Haven, Conn.
2376.	BRYANT, HENRY GRIER, F.R.G.S.	May 20, 1898,	Room 805 Land Title Building. Philadelphia.
2237.	BRYCE, RIGHT HON. JAMES . . .	Feb'y 15, 1895,	54 Portland Place, London, W., England.
2236.	BUDGE, E. A. WALLIS, Litt.D . .	Feb'y 15, 1895,	British Museum, London, Eng.
2007.	BURK, REV. JESSE Y	Jan'y 18, 1884,	400 Chestnut St., Philadelphia.
1938.	BUTLER, HON. WILLIAM	April 15, 1881,	West Chester, Pa.

C

2416.	CADWALADER, JOHN	May 19, 1899,	1519 Locust St., Philadelphia.
1788.	CAMPBELL, JOHN LYLE, Ph.D., Prof.	July 16, 1875,	Crawfordsville, Ind.

	Name.	*Date of Election.*	*Present Address.*
2492.	CAMPBELL, WM. W., Sc.D., LL.D.	April 3, 1903,	Lick Observatory, Mt. Hamilton, California.
1606.	CANBY, WILLIAM MARRIOTT . . .	Oct. 16, 1868,	1101 Delaware Avenue, Wilmington, Del.
2051.	CANNIZZARO, TOMASO	Oct. 16, 1885,	Santa Maria fuori cinta, Casa Roffa, Messina, Sicily.
1731.	CAPELLINI, GIOVANNI, Prof. . .	April 18, 1873,	Portovenere près Spezia, Italy.
1796.	CARLL, JOHN F., Prof.	Oct. 15, 1875,	Pleasantville, Venango Co., Pa.
2469.	CARNEGIE, ANDREW, LL.D . . .	April 4, 1902,	2 East 91st St., New York.
1911.	CARSON, HAMPTON L., LL.D. . .	April 16, 1880,	1033 Spruce St., Philadelphia.
2260.	CARTER, HON. JAMES C	May 17, 1895,	54 Wall Street, New York City.
1707.	CASSATT, ALEXANDER J	Oct. 18, 1872,	Haverford, Delaware Co., Pa.
2147.	CASTNER, SAMUEL, JR.	Dec. 16, 1887,	3729 Chestnut St., Philadelphia.
2152.	CATTELL, J. MCKEEN, Prof. . . .	May 18, 1888,	Garrison-on-Hudson, N. Y.
1908.	CHANCE, HENRY MARTYN, M.D .	April 16, 1880,	819 Drexel Building, Phila.
1783.	CHANDLER, C. F, Prof.	April 16, 1875,	Columbia Univ., N. Y. City.
1778.	CHAPMAN, HENRY C., M.D. . . .	April 16, 1875,	2047 Walnut St., Philadelphia.
2132.	DE CHARENCEY, COMTE HYACINTH	Dec. 17, 1886,	25 Rue Barbet de Jouy, Paris, France.
2158.	CLARK, CLARENCE H.	May 17, 1889,	42d and Locust Sts., Phila.
2470.	CLARK, WILLIAM BULLOCK, Prof.	April 4, 1902,	Johns Hopkins University, Baltimore, Md.
1983.	CLAYPOLE, E. W., Prof.	Jan'y 19, 1883,	Pasadena, Cal.
2247.	CLEEMANN, RICHARD A., M.D . .	Feb'y 15, 1895,	2135 Spruce St., Philadelphia.
2336.	CLEVELAND, HON. GROVER . . .	Oct. 15, 1897,	Westland, Princeton, N. J.
1999.	COHEN, J. SOLIS, M.D.	Jan'y 18, 1884,	1824 Chestnut St., Philadelphia.
2429.	COLES, EDWARD	Dec. 15, 1899,	2010 DeLancey Place, Phila.
2475.	COLLITZ, HERMAN, LL.D., Prof .	April 4, 1902,	Bryn Mawr, Pa.
2305.	CONKLIN, EDWIN GRANT, Prof. .	Feb'y 19, 1897,	University of Penna., Phila.
2386.	CONVERSE, JOHN H.	May 20, 1898,	500 N. Broad St., Philadelphia.
2257.	COOK, JOEL	May 17, 1895,	849 N. Broad St., Philadelphia.
2129.	CORA, GUIDO, Prof.	Dec. 17, 1886,	2 Via Goito, Rome, Italy.
2205.	CRAMP, CHARLES H	Dec. 16, 1892,	507 S. Broad St., Philadelphia.
1836.	CRANE, THOMAS FREDERICK, Prof.	Feb'y 2, 1877,	Cornell Univ., Ithaca, N. Y.
2100.	CROOKES, SIR WILLIAM	May 21, 1886,	7 Kensington Park Gardens, London, W., England.
2391.	CROWELL, EDWARD P., Prof. . .	Dec. 16, 1898,	21 Amity St., Amherst, Mass.
2317.	CULIN, STEWART	May 21, 1897,	Brooklyn Institute of Arts and Sciences, Brooklyn, N. Y.

D

2361.	DALL, WILLIAM H., Prof.	Dec. 17, 1897,	U. S. National Museum, Washington, D. C.
2402.	DANA, CHARLES E.	May 19, 1899,	2013 DeLancey Place, Philadelphia.
2282.	DANA, EDWARD S., Prof.	May 15, 1896,	Yale Univ., New Haven, Conn.
1808.	DANNEFELD, C. JUHLIN	April 21, 1876,	Stockholm, Sweden.
2480.	DARBOUX, JEAN-GASTON	April 4, 1902,	36 Rue Gay-Lussac, Paris, France
2369.	DARWIN, GEORGE HOWARD, Prof.	Feb'y 18, 1898,	Newham Grange, Cambridge, England.
1811.	DAVENPORT, SIR SAMUEL	Oct. 20, 1876,	Beaumont, Adelaide, S. Australia.
1557.	DAVIDSON, GEORGE, Prof.	Jan'y 19, 1866,	2221 Washington St., San Francisco, Cal.
2417.	DAVIS, WILLIAM MORRIS, Prof. .	Oct. 20, 1899,	Cambridge, Mass.
1923.	DAWKINS, WILLIAM BOYD, Prof.	Oct. 15, 1880,	Woodhurst, Fallowfield, Manchester, England.

	Name.	*Date of Election.*	*Present Address.*
2418.	DAY, FRANK MILES	Oct. 20, 1899,	801 Penn Mutual Building, Philadelphia.
2406.	DAY, WILLIAM C., Prof.	May 19, 1899,	Swarthmore, Pa.
2360.	DE GARMO, CHARLES, Prof. . . .	Dec. 17, 1897,	Cornell Univ., Ithaca, N. Y.
2208.	DERCUM, FRANCIS X., M.D. . . .	Dec. 16, 1892,	1719 Walnut St., Philadelphia.
2434.	DEWAR, JAMES, LL.D., Prof. . . .	Dec. 15, 1899,	The Royal Institution, London, England.
2013.	DICKSON, SAMUEL	April 18, 1884,	901 Clinton St., Philadelphia.
2206.	DIXON, SAMUEL G., M.D	Dec. 16, 1892,	Black Rock Farm, Ardmore, Pa.
2504.	DOHRN, ANTON, Dr.	April 3, 1903,	Marine Zoological Station, Naples, Italy.
2108.	DOLLEY, CHARLES S., M.D. . . .	Dec. 17, 1886,	3707 Woodland Ave., Phila.
2089.	DONNER, OTTO, Prof.	May 21, 1886,	Helsingfors, Finland.
1946.	DOOLITTLE, C. L., Prof.	Oct. 21, 1881,	Upper Darby, Delaware Co., Pa.
2493.	DOOLITTLE, ERIC	April 3, 1903,	University of Pennsylvania, Philadelphia.
2425.	DOUGHERTY, THOMAS HARVEY .	Dec. 15, 1899,	School House Lane, Germantown, Philadelphia.
1839.	DOUGLAS, JAMES, LL D.	April 20, 1877,	Spuytenduyvil, New York, N.Y.
1924.	DRAPER, DANIEL, Ph.D	Oct. 15, 1880,	Meteorological Observatory, Central Park, New York.
1787.	DROWN, THOMAS M., Pres't . . .	July 16, 1875,	Lehigh Univ., S. Bethlehem, Pa.
1918.	DU BOIS, PATTERSON	Oct. 15, 1880,	401 S. 40th St., Philadelphia.
1878.	DUDLEY, CHARLES BENJ., Ph.D. .	Jan'y 17, 1879,	Drawer 334, Altoona, Blair Co., Pa.
2063.	DUNCAN, LOUIS, Ph.D., U.S.N. . .	Feb'y 19, 1886,	Mass. Institute of Technology, Boston.
1573.	DUNNING, GEORGE F.	Jan'y 18, 1867,	Farmington, Conn.
1727.	DUPONT, EDOUARD	April 18, 1873,	Royal Museum, Bruxelles, Belgium.
2227.	DUPONT, HENRY A., Col.	Feb'y 16, 1894,	Winterthur, Del.
1679.	DUTTON, CLARENCE E., Maj. U.S.A.	Jan'y 20, 1871,	Morgan Park, Cook Co., Ill.

E

2105.	EASTON, MORTON W., Prof. . . .	Dec. 17, 1886,	224 S. 43d St., Philadelphia.
1917.	ECKFELDT, JACOB B	Oct. 15, 1880,	U. S. Mint, Philadelphia.
1825.	EDDY, H. TURNER, Prof	Feb'y 2, 1877,	University of Minnesota, Minneapolis, Minn.
2294.	EDISON, THOMAS A., Ph.D	May 15, 1896,	Orange, N. J.
2262.	EDMUNDS, HON. GEORGE F. . . .	May 17, 1895,	1724 Spruce St., Philadelphia.
1686.	ELIOT, CHARLES W., Pres't	April 21, 1871,	17 Quincy St., Cambridge, Mass.
2272.	ELLIOTT, A. MARSHALL, Prof. . .	May 17, 1895,	Johns Hopkins University, Baltimore, Md.
2313.	ELY, THEODORE N.	May 21, 1897,	115 Broad St. Station, Phila.
2356.	EMERSON, BENJ. KENDALL, Prof.	Dec. 17, 1897,	Amherst, Mass.
2368.	EMMET, W. L. R	Feb'y 18, 1898,	48 Washington Ave., Schenectady, N. Y.
1981.	EMMONS, S. F., Prof.	Jan'y 19, 1883,	1721 H St., Washington, D. C.
1943.	EVANS, SIR JOHN, K.C.B	Oct. 21, 1881,	Nash Mills, Hemel Hempstead, England.
2254.	EWELL, MARSHALL D., M.D., LL.D.	May 17, 1895,	59 Clark St., Chicago, Ill.

F

2234.	FENNELL, C. A. M., Litt.D. . . .	Feb'y 15, 1895,	139 Chesterton Road, Cambridge, England.
2180.	FIELD, ROBERT PATTERSON . . .	May 16, 1890,	218 S. 42d St., Philadelphia.
2364.	FINE, HENRY B., Prof	Dec. 17, 1897,	Princeton, N. J.

	Name.	*Date of Election.*	*Present Address.*
2353.	FISHER, SYDNEY GEORGE	Dec. 17, 1897,	828 Chestnut St., Philadelphia.
2462.	FLEXNER, SIMON, Dr.	Feb'y 15, 1901,	University of Pennsylvania, Philadelphia.
1901.	FLINT, AUSTIN, M.D.	April 16, 1880,	60 E. 34th St., New York, N.Y.
2197.	FORBES, GEORGE, Prof., F.R.S. .	Oct. 16, 1891,	84 GreatGeorge St., S.W. London.
2487.	FOSTER, SIR MICHAEL, K.C.B., F.R.S., D.C.L	April 4, 1902,	Nine Wells, Great Shelford, Cambridge, Eng.
1912.	FRALEY, JOSEPH C.	April 16, 1880,	1833 Pine St., Philadelphia.
1695.	FRAZER, PERSIFOR, Dr. ès-Sc. Nat.	Jan'y 19, 1872,	928 Spruce St., Philadelphia.
2301.	FRAZIER, BENJ. W., Prof. . . .	Dec. 18, 1896,	Lehigh Univ., Bethlehem, Pa.
2171.	FRIEBIS, GEORGE, M.D.	Dec. 20, 1889,	1906 Chestnut St., Philadelphia.
2179.	FULLERTON, GEORGE S., Rev. . .	May 16, 1890,	89, The Gladstone, Philadelphia.
1789.	FULTON, JOHN	April 18, 1878,	136 Park Pl., Johnstown, Pa.
1914.	FURNESS, HORACE Howard, LL.D.	April 16, 1880,	Wallingford, Delaware Co., Pa.
2306.	FURNESS, HORACE HOWARD, JR..	Feb'y 19, 1897,	2034 DeLancey Place, Phila.
2304.	FURNESS, WILLIAM H., 3d, M.D. .	Feb'y 19, 1897,	1906 Sansom St., Philadelphia.

G

2459.	GARNETT, RICHARD, C.B., LL.D..	Feb'y 15, 1901,	27 Tanza Road, Hampstead, London, England.
1988.	GARRETT, PHILIP C	April 20, 1883,	Logan P. O., Philadelphia.
2079.	GATES, MERRILL E., LL.D . . .	May 21, 1886,	1315 New Hampshire Ave., Washington, D. C.
1025.	GATSCHET, ALBERT S., Ph.D . . .	Oct. 17, 1884,	2020 Fifteenth St., N. W., Washington, D. C.
1897.	GEIKIE, SIR ARCHIBALD	Jan'y 16, 1880,	28 Jermyn St., London, S. W., England.
1803.	GEIKIE, JAMES, Prof.	April 21, 1876,	31 Merchiston Ave., Edinburgh, Scotland.
2067.	GENTH, F. A., JR	Feb'y 19, 1886,	103 N. Front St., Philadelphia
1355.	GIBBS, OLIVER WOLCOTT, Prof. .	July 21, 1854,	158 Gibbs Ave., Newport, R. I.
2458.	GIGLIOLI, HENRY H., Prof. . . .	Feb'y 15, 1901,	19 Via Romana, Florence, Italy.
2483.	GILBERT, GROVE K., LL.D. . . .	April 4, 1902,	U. S. Geological Survey, Washington, D. C.
2494.	GILDERSLEEVE, BASIL L., LL.D., Prof.	April 3, 1903,	1002 Belvidere Terrace, Baltimore, Md.
1587.	GILL, THEODORE N., M.D., Ph.D .	July 19, 1867,	Smithsonian Inst., Washington, D. C.
1800.	GILMAN, DANIEL C., LL.D	April 21, 1876,	614 Park Ave., Baltimore, Md.
2233.	GLAZEBROOK, RICHARD T., F.R.S.	Feb'y 15, 1895,	Bushey House, Teddington, Middlesex, Eng.
2212.	GOODALE, GEORGE LINCOLN, Prof	Feb. 17, 1893,	10 Craigie St., Cambridge, Mass.
2292.	GOODSPEED, ARTHUR W., Prof.	May 15, 1896,	Univ. of Pennsylvania, Philadelphia.
2203.	GOODWIN, HAROLD.	May 20, 1892,	133 S. 12th St., Philadelphia.
2232.	GOODWIN, W. W., Prof.	Feb'y 15, 1895,	Cambridge, Mass.
2453.	GRAY, GEORGE, Hon.	May 18, 1900,	Wilmington, Del.
2222.	GREEN, SAMUEL A., M.D.	Oct. 20, 1893,	Historical Soc., Boston, Mass.
1880.	GREENE, WILLIAM H., M.D. . . .	April 18, 1879,	N. E. Cor. Arch and 16th Sts., Philadelphia.
2412.	GREENMAN, MILTON J., M.D. . .	May 19, 1899,	Wistar Institute, 36th and Darby Road, Philadelphia.
2155.	DI GREGORIO, MARCHESE ANTONIO	Dec. 21, 1888,	Al Molo, Palermo, Sicily.

	Name.	*Date of Election.*	*Present Address.*
2188.	GREGORY, CASPAR RÉNÉ, Prof. .	May 15, 1891,	Naunhofstrasse 25, Marienhöhe, Leipzig-Stotteritz, Germany.
2090.	DE GUBERNATIS, ANGELO, Prof. .	May 21, 1886,	Florence, Italy.
2495.	GUMMERE, FRANCIS BARTON, Ph.D., Prof.	April 3, 1903,	Haverford College, Haverford, Pa.
	H		
2479.	HADLEY, ARTHUR T., Pres't. . .	April 4, 1902,	Yale University, New Haven, Conn.
2054.	HAECKEL, ERNST, Prof.	Oct. 16, 1885,	University, Jena, Germany.
2496.	HAGUE, ARNOLD, D.Sc.	April 3, 1903,	1724 I St., Washington, D. C.
1658.	HALE, REV. EDWARD EVERETT, .	Jan'y 21, 1870,	89 Highland St., Roxbury, Mass.
2477.	HALE, GEORGE E., Prof.	April 4, 1902,	Yerkes Observatory, Williams Bay, Wis.
1853.	HALL, ASAPH, Prof.	Jan'y 18, 1878,	South Norfolk, Conn.
1795.	HALL, CHARLES EDWARD.	Oct. 15, 1875,	Instituto Geologico de Mexico, Santa Maria, Mexico, Mexico.
2396.	HALL, CHARLES M	Dec. 16, 1898,	Niagara Falls, N. Y.
2027.	HALL, LYMAN B., Prof.	Jan'y 16, 1885,	Haverford Coll., Haverford, Pa.
2194.	HAMY, E. T., Dr	May 15, 1891,	40 Rue Lübeck, Ave. du Trocadero, Paris, France.
2136.	HARRIS, JOSEPH S.	May 20, 1887,	144 School Lane, Germantown, Philadelphia.
2246.	HARRISON, CHARLES C., Provost.	Feb'y 15, 1895,	400 Chestnut St., Philadelphia.
1827.	HART, JAMES MORGAN, Prof. . .	Feb'y 2, 1877,	1 Reservoir Ave., Ithaca, N. Y.
2365.	HATCHER, JOHN B., Prof. . . .	Dec. 17, 1897,	Carnegie Museum, Pittsburgh, Pa.
1681.	HAUPT, HERMANN, Gen	April 21, 1871,	The Concord, Washington, D.C.
1862.	HAUPT, LEWIS M., Prof.	May 3, 1878,	107 N. 35th St., Philadelphia.
2481.	HAUPT, PAUL, Prof.	April 4, 1902,	2511 Madison Ave., Baltimore.
2446.	HAY, JOHN, Hon.	Dec. 16, 1898,	State Dep't, Washington, D.C.
2082.	HAYES, RICHARD SOMERS, Capt .	May 21, 1886,	32 Nassau St., New York.
2071.	HAYS, I. MINIS, M.D.	Feb'y 19, 1886,	266 S. 21st St., Philadelphia.
1985.	HEILPRIN, ANGELO, Prof	April 20, 1883,	1801 Arch St., Philadelphia.
2218.	HEWETT, WATERMAN T., Prof. .	May 19, 1893,	31 East Ave., Ithaca, N. Y.
2266.	HEYSE, PAUL, Ph.D.	May 17, 1895,	Munich, Bavaria.
2497.	HILL, GEORGE WILLIAM, LL.D. .	April 3, 1903,	West Nyack, N. Y.
2307.	HILLER, H. M., M.D.	Feb'y 19, 1897,	1510 Walnut St., Philadelphia.
2110.	HILPRECHT, HERMANN V., Prof .	Dec. 17, 1886,	403 S. 41st St., Philadelphia.
1768.	HIMES, CHARLES FRANCIS, Prof. .	Oct. 16, 1874,	Dickinson Coll., Carlisle, Pa.
2438.	HIRST, BARTON COOKE, M.D. . .	Dec. 15, 1899,	1821 Spruce St., Philadelphia.
1663.	HITCHCOCK, CHAS. HENRY, Prof.	April 15, 1870,	Dartmouth Coll., Hanover, N.H.
2355.	HOLDEN, EDWARD S., Prof. . . .	Dec. 17, 1897,	U. S. Military Academy, West Point, N. Y.
2068.	HOLLAND, JAMES W., M.D. . . .	Feb'y 19, 1886,	2006 Chestnut St., Philadelphia.
2440.	HOLMES, WILLIAM H., Prof. . . .	Dec. 15, 1899,	Bureau of Ethnology, U. S. National Museum, Washington, D. C.
1624.	HOOKER, SIR JOSEPH D., LL.D . .	Jan'y 15, 1869,	The Camp, Sunningdale, Eng.
2224.	HOPPIN, J. M., Prof.	Oct. 20, 1893,	New Haven, Conn.
2070.	HORNER, INMAN.	Feb'y 19, 1886,	1811 Walnut St., Philadelphia.
1696.	HOUGH, GEORGE W., Prof. . . .	Jan'y 19, 1872,	N.W. University, Evanston, Ill.
1698.	HOUSTON, EDWIN J., Prof. . . .	Jan'y 19, 1872,	1809 Spring Garden St., Phila.
2346.	HOWE, HENRY M., Prof.	Oct. 15, 1897,	27 W. 73d St., New York.

	Name.	*Date of Election.*	*Present Address.*
2498.	HOWELL, WILLIAM HENRY, Ph.D., Prof.	April 3, 1903,	232 West Lanvale St., Baltimore.
2239.	HUGGINS, SIR WILLIAM, K.C.B.	Feb'y 15, 1895,	90 Upper Tulse Hill, S.W., London, England.
1843.	HUMPHREY, H. C.	July 20, 1877,	?
2248.	HUNTER, RICHARD S.	Feb'y 15, 1895,	1413 Locust St., Philadelphia.
2373.	HUTCHINSON, EMLEN	May 20, 1898,	Aldine Hotel, Philadelphia.

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1773.	INGHAM, WM. ARMSTRONG.	April 16, 1875,	320 Walnut St., Philadelphia.
2217.	D'INVILLIERS, EDWARD VINCENT.	May 19, 1893,	506 Walnut St., Philadelphia.

J

2010.	JAMES, EDMUND J., Pres't.	April 18, 1884,	5833 Monroe Ave, Chicago, Ill.
2302.	JASTROW, MORRIS, JR., Prof.	Feb. 19, 1897,	248 S. 23d St., Philadelphia.
2375.	JAYNE, HENRY LABARRE, LL D.	May 20, 1898,	1826 Chestnut St., Phila.
2049.	JAYNE, HORACE, M.D.	Oct. 16, 1885,	318 S. 19th St., Philadelphia.
1954.	JEFFERIS, WILLIAM W	Jan'y 20, 1882,	442 Central Park West, New York City.
2017.	JORDAN, FRANCIS, JR.	April 18, 1884,	111 N. Front St., Philadelphia.

K

1989.	KANE, ELISHA KENT.	April 20, 1883,	Kushequa, Pa.
2322.	KARPINSKY, ALEX. PETROVITCH, Prof.	May 21, 1897,	Geological Survey, St. Petersburg, Russia.
2169.	KEANE, JOHN J., Right Rev.	Dec. 20, 1889,	Dubuque, Iowa.
2422.	KEASBEY, LINDLEY M., Prof.	Dec. 15, 1899,	Bryn Mawr, Pa.
2329.	KEEN, GREGORY B.	Oct. 15, 1897,	2320 Spruce St., Philadelphia.
2021.	KEEN, WILLIAM W., M.D., LL.D.	July 18, 1884,	1729 Chestnut St., Philadelphia.
2392.	KEISER, EDWARD H., Prof.	Dec. 16, 1898,	Washington University, St. Louis, Mo.
2450.	KELLER, HARRY F., Prof.	May 18, 1900,	Central High School, Phila.
1728.	KELVIN, RIGHT HON. LORD	April 18, 1873,	The Library, The University, Glasgow, Scotland.
2278.	KENNELLY, A. E., D.Sc	Feb. 28, 1896,	Harvard University, Cambridge, Mass.
2392.	KNIGHT, WILLIAM A., Prof.	Dec. 16, 1898,	St. Andrew's, Scotland.
1767.	KÖNIG, GEORGE A., Prof.	Oct. 16, 1874,	School of Mines, Houghton, Mich.
2424.	KRAEMER, HENRY, Prof.	Dec. 15, 1899,	145 N. 10th St., Philadelphia.
2167.	KRAUSS, FRIEDRICH S., Ph.D.	Dec. 20, 1889,	VII² Neustiftgasse 12, Vienna, Austria.

L

1694.	LAMBERT, GUILLAUME, Prof.	Jan'y 19, 1872,	42 Boulevard Bischoffsheim, Brussels, Belgium.
2411.	LAMBERTON, WILLIAM A., Prof	May 19, 1899,	University of Penna., Phila.
2377.	DE LANCEY, EDWARD F	May 20, 1898,	20 E. 28th St., New York.
2344.	LANCIANI, RUDOLFO, Prof	Oct. 15, 1897,	2 Via Goito, Rome, Italy.
1858.	LANDRETH, BURNET.	Jan'y 18, 1878,	Bristol, Pa.
1781.	LANGLEY, SAMUEL P., LL.D	April 16, 1875,	Smithsonian Institution, Washington, D. C.
2505.	LANKESTER, EDWIN RAY. LL.D., F.R.S.	April 3, 1903,	British Museum, Cromwell Rd., London, S. W., Eng.

	Name.	*Date of Election.*	*Present Address.*
1721.	La Roche, C. Percy, M.D. . . .	Jan'y 17, 1873,	1518 Pine Street, Philadelphia.
1595.	Lea, Henry Charles, LL.D. . .	Oct. 18, 1867,	2000 Walnut St., Philadelphia.
2407.	Learned, Marion D., Prof . . .	May 19, 1899,	University of Penna., Phila.
1986.	Lehman, Ambrose E.	April 20, 1883,	506 Walnut St., Philadelphia.
2174.	Le Moine, Sir James M.	Dec. 20, 1889,	Spencer Grange, Quebec, Canada.
1931.	Leroy-Beaulieu, Paul, Prof. .	April 15, 1881,	27 Ave. du Bôis de Boulogne, Paris, France.
2085.	Levasseur, Emile, Prof.	May 21, 1886,	26 Rue Mons. le Prince, Paris, France.
2300.	Lewis, G. Albert	Dec. 18, 1896,	1834 DeLancey Place, Phila.
2338.	Libbey, William, Prof.	Oct. 15, 1897,	20 Bayard Ave., Princeton, N.J.
2492.	Lippincott, J. Dundas	Dec. 15, 1899,	1333 Walnut St., Philadelphia.
2312.	Lister, The Right Hon. Lord .	May 21, 1897,	12 Park Crescent, Portland Place, London, England.
1756.	Lockyer, Sir Joseph Norman, K.C.B	April 17, 1874,	Royal College of Science, S. Kensington, London, S. W., England.
2160.	Lodge, Sir Oliver Joseph, LL.D.	Feb'y 15, 1901,	The University, Birmingham, England.
2435.	Loeb, Jacques, Dr.	Dec. 15, 1899,	University of California, Berkeley, Cal.
1872.	Longstreth, Morris, M.D. . . .	Sept. 20, 1878,	1416 Spruce St., Philadelphia.
2202.	Low, Hon. Seth	Feb. 19, 1892,	30 E. 46th St., New York.
2350.	Lowell, Percival	Oct. 15, 1897,	53 State St., Boston.
1629.	Lyman, Benjamin Smith	Jan'y 15, 1869,	708 Locust St., Philadelphia.

M

2319.	Mabery, Charles F., Prof. . . .	May 21, 1897,	57 Adelbert St., Cleveland, O.
2107.	MacAlister, James, Pres't. . . .	Dec. 17, 1886,	119 N. 18th St., Philadelphia.
2207.	Macfarlane, John M., Prof. . .	Dec. 16, 1892,	Lansdowne, Delaware Co., Pa.
2404.	Mackenzie, Arthur S., Prof. . .	May 19, 1899,	Bryn Mawr, Pa.
2363.	McCay, Leroy W., Prof.	Dec. 17, 1897,	Princeton, N. J.
2366.	McClure, Charles F. W., Prof.	Dec. 17, 1897,	Princeton, N. J.
2280.	McCook, Henry C., Rev., D.D. .	Feb. 23, 1896,	3700 Chestnut St., Philadelphia.
1888.	McCreath, Andrew S.	July 18, 1879,	223 Market St., Harrisburg, Pa.
2299.	Magie, Wm. Francis, Prof. . . .	Dec. 18, 1896,	Princeton, N. J.
2339.	Mahan, Alfred T., Capt. U.S.N.	Oct. 15, 1897,	160 W. 86th St., New York.
2012.	Mallet, John Wm., M.D. . . .	Jan'y 16, 1885,	University of Virginia, Charlottesville, Va.
1847.	Mansfield, Ira Franklin . . .	Jan'y 18, 1878,	Beaver, Beaver Co., Pa.
1857.	March, Francis Andrew, Prof.	Jan'y 18, 1878,	Lafayette College, Easton, Pa.
2461.	Marconi, Guglielmo	Feb'y 15, 1901,	The Haven Hotel, Sand Barths, Poole, Dorset, England.
2463.	Marcovnikoff, Vladimir, Prof..	Feb'y 15, 1901.	Imp. Moskovskÿ Universitet, Moscow, Russia.
1861.	Marks, William D., Prof. . . .	May 3, 1878,	Westport, Essex Co., N. Y.
2078.	Marshall, John, M.D	May 21, 1886,	1718 Pine St., Philadelphia.
2184.	Mascart, E., Prof.	Dec. 19, 1890,	176 Rue de l'Université, Paris, France.
1572.	Mason, Andrew	Jan'y 18, 1867,	30 and 32 Wall St., New York.
2431.	Mason, Otis T., Prof.	Dec. 15, 1899,	U. S. National Museum, Washington, D. C.
2279.	Mason, Wm. Pitts, M.D., Prof. .	Feb. 28, 1896,	Rensselaer Polytechnic Institute, Troy, N. Y.
2196.	Maspero, Gaston Camille, Prof.	May 15, 1891,	Paris, France.

	Name.	*Date of Election.*	*Present Address.*
2427.	MATTHEWS, ALBERT.	Dec. 15, 1899,	145 Beacon St., Boston.
2399.	MEIGS, ARTHUR V., M.D.	May 19, 1899,	1322 Walnut St., Philadelphia.
2456.	MEIGS, WILLIAM M	Feb'y 15, 1901,	1815 Pine St., Philadelphia.
2115.	VON MELTZEL, HUGO, Prof. Dr. .	Dec. 17, 1886,	Koloszvar, Hungary.
2330.	MELVILLE, GEO. W., Rear Admiral.	Oct. 15, 1897,	Navy Dept., Washington, D. C.
2430.	MENDENHALL, THOMAS C, Prof.	Dec. 15, 1899,	Worcester, Mass.
2387.	MENGARINI, GUGLIELMO, Prof. .	May 20, 1898,	Rome, Italy.
2251.	MERCER, HENRY C.	Feb. 15, 1895,	Doylestown, Pa.
2485.	MERRIAM, C. HART, Dr.	April 4, 1902,	U. S. Biological Survey, Dep't of Agriculture, Washington, D. C.
1903.	MERRICK, JOHN VAUGHAN.	April 16, 1880,	Roxborough, Philadelphia.
1947.	MERRIMAN, MANSFIELD, Prof. .	Oct. 21, 1881,	Lehigh Univ., Bethlehem, Pa.
1744.	MESSCHERT, MATTHEW HUIZINGA.	Oct. 17, 1873,	Douglassville, Berks Co., Pa.
2436.	MEYER, ADOLPH B., Prof.	Dec. 15, 1899,	K. Zoölogisches u. Anthropologisch-Ethnographisches Museum, Dresden, Germany.
2142.	MICHAEL, MRS. HELEN ABBOTT .	May 20, 1887,	140 Beacon St., Boston.
2484.	MICHELSON, ALBERT A., Prof., Sc.D. (Cantab)	April 4, 1902,	University of Chicago, Chicago, Ill.
2423.	MILLER, LESLIE W., Prof.	Dec. 15, 1899,	N. W. cor. Broad and Pine Sts., Philadelphia.
2284.	MINOT, CHAS. SEDGWICK, M.D. .	May 15, 1896,	Harvard Univ., Cambridge, Mass.
2175.	MITCHELL, HON. JAMES T. . . .	Feb'y 21, 1890,	1722 Walnut St., Philadelphia.
1461.	MITCHELL, S. WEIR, M.D	Jan'y 17, 1862,	1524 Walnut St., Philadelphia.
2267.	MONTEGAZA, PAOLO	May 17, 1895,	Florence, Italy.
2367.	MONTGOMERY, THOS. H., Jr., Prof.	Feb'y 18, 1898,	Univ. of Texas, Austin, Texas.
2323.	MOORE, CLARENCE B.	Oct. 15, 1897,	1321 Locust Street, Phila.
2029.	MOORE, JAMES W., M.D.	Jan'y 16, 1885,	Lafayette College, Easton, Pa.
1841.	MOREHOUSE, GEORGE R., M.D. .	April 20, 1877,	2033 Walnut St., Philadelphia.
2499.	MORLEY, EDWARD W., Ph.D., LL.D.	April 8, 1903,	Adelbert College, Cleveland, Ohio.
2340.	MORLEY, FRANK, Prof.	Oct. 15, 1897,	Johns Hopkins University, Baltimore.
2409.	MORRIS, HARRISON S.	May 19, 1899,	Academy of Fine Arts, Philadelphia.
2397.	MORRIS, ISRAEL W.	May 19, 1899,	225 So. 8th St., Philadelphia.
1976.	MORRIS, J. CHESTON, M.D.	Jan'y 19, 1883,	1514 Spruce St., Philadelphia.
2454.	MORRIS, JOHN T.	Feb'y 15, 1901,	826 Drexel Building, Phila.
2265.	MORSE, EDWARD S., Prof.	May 17, 1895,	Essex Institute, Salem, Mass.
2500.	MORSE, HARMON N., Ph.D. . . .	April 8, 1903,	1117 N. Eutaw St., Baltimore.
2121.	MUCH, MATHÆUS, Ph.D., Prof. .	Dec. 17, 1886,	XIII Penzingerstrasse, 84, Vienna, Austria.
2464.	MUNRO, DANA C., Prof.	May 17, 1901,	Univ. of Wisconsin, Madison, Wis.
2192.	MUNROE, CHARLES E., Prof. . .	May 15, 1891,	Columbian Univ., Washington, D. C.
2062.	MURDOCK, J. B., Com. U.S.N. . .	Feb'y 19, 1886,	Navy Dept., Washington, D.C.
1937.	MURRAY, JAMES A. H., LL.D . .	April 15, 1881,	Sunnyside, Banbury Road, Oxford, England.

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2087.	DE NADAILLAC, MARQUIS.	May 21, 1896,	18 Rue Duphot, Paris, France.
2316.	NANSEN, FRIDTJOF, Prof.	May 21, 1897,	Godthaab, Lysaker, Norway.
1852.	NEWCOMB, SIMON, Prof.	Jan'y 18, 1878,	1620 P St., Washington, D. C.

	Name.	*Date of Election.*	*Present Address.*
1703.	NICHOLS, STARR HOYT, Rev . . .	July 19, 1872,	128 Main St., Danbury, Conn.
2360.	NIKITIN, SERGI, Prof.	Feb'y 19, 1866,	Geological Survey, St. Petersburg, Russia.
1712.	NORRIS, ISAAC, M.D.	Oct. 18, 1872,	Fair Hill, Bryn Mawr, Pa.
2259.	NUTTALL, MRS. ZELIA	May 17, 1895,	Plaza de Alvarado, Coyoacán. D. F. Mexico.

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2372.	OLIVER, CHARLES A., M.D. . . .	Feb'y 19, 1896,	1507 Locust St., Philadelphia.
2354.	OLNEY, RICHARD, Hon	Dec. 17, 1897,	23 Court Street, Boston.
2195.	OPPERT, JULES, Prof	May 15, 1891,	2 Rue de Sfax, Paris, France.
2362.	ORTMANN, ARNOLD E., Prof. . . .	Dec. 17, 1897,	Carnegie Museum, Shenley Park, Pittsburg, Pa.
2135.	OSBORN, HENRY F., Prof.	Feb'y 18, 1887,	American Museum of Natural History, New York.
2039.	OSLER, WILLIAM, M.D	Jan'y 16, 1885,	1 West Franklin St., Baltimore, Md.

P

1868.	PACKARD, ALPHEUS S., Prof. . .	Sept. 20, 1878,	Providence, R. I.
1578.	PACKARD, JOHN H., M.D.	Jan'y 18, 1867,	University Club, Philadelphia.
2395.	PANCOAST, HENRY S	Dec. 16, 1898,	267 E. Johnson St., Germantown, Phila.
2035.	PATTERSON, C. STUART.	Jan'y 16, 1885,	1000 Walnut St., Philadelphia.
2452.	PATTERSON, EDWARD, Hon. . . .	May 18, 1900,	Supreme Court, Appellate Div., 1st Dept., New York City.
2385.	PATTERSON, LAMAR GRAY . . .	May 20, 1898,	Guano, Amherst Co., Va.
1282.	PATTERSON, ROBERT	April 18, 1851,	329 Chestnut St., Philadelphia.
1320.	PATTERSON, THOMAS LEIPER . .	April 15, 1853,	176 Washington St., Cumberland, Md.
2218.	PATTISON, ROBERT E., Hon . . .	Feb. 17, 1893,	5930 Drexel Rd., Overbrook, Pa.
2357.	PATTON, FRANCIS L., D.D., Pres't	Dec. 17, 1897,	Princeton, N. J.
2428.	PAUL, J. RODMAN	Dec. 15, 1899,	903 Pine St., Philadelphia.
1772.	PEARSE, JOHN B.	Jan'y 15, 1875,	317 Walnut Av., Roxbury, Mass.
2318.	PECKHAM, S. F., Prof	May 21, 1897,	49 Pacific St., Brooklyn.
1859.	PEIRCE, C. NEWLIN, D.D.S. . . .	May 3, 1878.	3316 Powelton Ave., Philadelphia.
1722.	PEMBERTON, HENRY.	Jan'y 17, 1873,	1947 Locust St., Philadelphia.
2101.	PEÑAFIEL, ANTONIO, Dr	May 21, 1886,	Ciudad Mexico, Mexico.
2455.	PENNIMAN, JOSIAH H., Prof. . .	Feb'y 15, 1901,	4326 Sansom St., Philadelphia.
2073.	PENNYPACKER, SAMUEL W., Hon.	May 21, 1886,	Executive Mansion, Harrisburg, Pa.
1518.	PENROSE, R. A. F., M.D.	July 17, 1863,	1331 Spruce St., Philadelphia.
2059.	PEPPER, EDWARD, M.D.	Feb'y 19, 1886,	El Afia, El Biar, Alger, Algerie.
2333.	PEPPER, GEORGE WHARTON, LL.D.	Oct. 15, 1897,	701 Drexel Building, Phila.
2383.	PETTEE, WILLIAM HENRY, Prof.	May 20, 1898,	554 Thompson St., Ann Arbor, Mich.
2281.	PETTIT, HENRY	Feb. 28, 1895,	5951 Overbrook Ave., Philadelphia.
2403.	PHILLIPS, FRANCIS C., Prof. . .	May 19, 1899,	P. O. Box 126, Allegheny, Pa.
2295.	PICKERING, EDW. C., Prof. . . .	May 15, 1896,	Harvard Univ., Cambridge, Mass.
2342.	PIERSOL, GEORGE A., M.D. . . .	Oct. 15, 1897,	Chester Ave. and 49th St., Philadelphia.
2277.	PILSBRY, HENRY A., Prof. . . .	Dec. 20, 1895,	Academy of Natural Sciences, Philadelphia.
2374.	PLATT, CHARLES.	May 20, 1898,	237 S. 18th St., Philadelphia.

	Name.	*Date of Election.*	*Present Address.*
2415.	POINCARÉ, HENRI, Prof.	May 19, 1899,	63 Rue Claude Bernard, Paris, France.
2053.	POMIALOWSKY, JOHN, Prof. . . .	Oct. 16, 1885,	St. Petersburg, Russia.
2097.	POSTGATE, JOHN P., Prof.	May 21, 1886,	Cambridge, England.
2437.	PREECE, SIR WM. HENRY, F.R.S.	Dec. 15, 1899,	12, Queen Anne's Gate, London, S. W., England.
2382.	PRESCOTT, ALBERT B., LL.D., Prof.	May 20, 1898,	734 S. Ingalls St., Ann Arbor, Mich.
1780.	PRIME, FREDERICK	April 16, 1875,	1008 Spruce St., Philadelphia.
2414.	PRITCHETT, HENRY S, LL.D., President	May 19, 1899,	Massachusetts Institute of Technology, Boston.
1758.	PUMPELLY, RAPHAEL, Prof. . . .	April 17, 1874,	Newport, R. I.
2293.	PUPIN, MICHAEL I., Prof. . .	May 15, 1896,	7 Highland Pl., Yonkers, N. Y.
2268.	PUTNAM, FREDERICK W., Prof. .	May 15, 1895,	Peabody Museum, Cambridge, Mass.

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2131.	RADA, JUAN DE DIOS-Y DELGADO,	Dec. 17, 1886,	Calle de la Corredera baja de S. Pablo No. 12, Madrid, Spain.
2401.	RAMSAY, SIR WILLIAM	May 19, 1899,	University College, Gower St. W. C., London, Eng.
1849.	RANDALL, F. A., M.D.	Jan'y 18, 1878,	Warren, Pa.
2105.	RAVENEL, MAZYCK P., Dr.	May 17, 1901,	University of Pennsylvania, Philadelphia.
2388.	RAWLE, FRANCIS.	Dec. 16, 1898,	328 Chestnut St., Philadelphia
2398.	RAWLE, WILLIAM BROOKE. . . .	May 19, 1899,	230 So. 22d St., Philadelphia.
2099.	RAYLEIGH, The Right Hon. Lord.	May 21, 1886,	Terling Pl., Witham, Essex, Eng.
1784.	RAYMOND, ROSSITER W.	April 16, 1875,	99 John St., New York, N. Y.
2381.	REDWOOD, BOVERTON	May 20, 1898,	4, Bishopsgate St. Within, E. C., London, England.
2405.	REMINGTON, JOSEPH P., Prof. . .	May 19, 1899,	1832 Pine St., Philadelphia.
1889.	REMSEN, IRA, President.	July 18, 1879,	Johns Hopkins Univ., Baltimore, Md.
1890.	RENEVIER, E., Prof.	July 18, 1879,	Univ., Lausanne, Switzerland.
2443.	RENNERT, HUGO A., Prof.	Dec. 15, 1899,	4232 Chestnut St., Philadelphia.
1816.	REULEAUX, F., Prof.	Feb'y 2, 1877,	W. Ahornstrasse 2, Berlin, Germany.
2122.	RÉVILLE, ALBERT, Prof.	Dec. 17, 1886,	21 Rue Guénégaud, Paris, France.
2472.	RICHARDS, THEO. WILLIAM, Prof.	April 4, 1902,	15 Follen St., Cambridge, Mass.
2226.	ROBERTS, ISAAC, Sc.D., F.R.S. . .	Oct. 20, 1893,	Starfield, Crowborough, Sussex, England.
1957.	ROBINS, JAMES W., Rev	April 21, 1892,	2115 Pine St., Philadelphia.
2177.	ROGERS, ROBERT W., Prof. . . .	Feb'y 21, 1890,	Drew Theological Seminary, Madison, N. J.
1462.	RÖHRIG, F. L. OTTO, Prof.	April 18, 1862,	4025 Oakland Ave., Pasadena, Cal.
2050.	ROLLETT, HERMANN, Ph.D. . . .	Oct. 16, 1885,	Baden bei Wien, Austria.
2506.	ROSCOE, SIR HENRY E., F.R.S., D.C.L.	April 3, 1903,	Woodcote Lodge, West Horsley. Leatherhead, England.
2198.	ROSENGARTEN, JOSEPH G.	Oct. 16, 1891,	1701 Walnut St., Philadelphia.
1964.	DE ROSNY, LÉON, Prof.	July 21, 1892,	28 Rue Mazarine, Paris, France
1838.	ROTHROCK, JOSEPH T., Prof. . .	April 20, 1877,	West Chester, Pa.

S

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2230.	SACHSE, JULIUS F., Litt.D.	Feb'y 16, 1894,	4428 Pine St., Phila.
1766.	SADTLER, SAMUEL P., Prof. . . .	Oct. 16, 1874,	N.E. cor. 10th and Chestnut Sts., Philadelphia.
2148.	SAJOUS, CHARLES E., M.D. . . .	Feb'y 17, 1888,	2043 Walnut St., Philadelphia.
2858.	SAMPSON, ALDEN.	Dec. 17, 1897,	Haverford, Pa.
1563.	SANDBERGER, FRIEDOLIN, Prof. .	April 20, 1866,	Univ. of Würzburg, Würzburg, Bavaria.
2327.	SANDERS, RICHARD H.	Oct. 15, 1897,	1225 Locust St., Philadelphia.
1956.	SARGENT, CHARLES SPRAGUE, Prof	April 21, 1882,	Jamaica Plain, Mass.
1730.	DE SAUSSURE, HENRI.	April 18, 1873,	Geneva, Switzerland.
3473.	SCHELLING, FELIX E., Ph.D., Prof.	April 4, 1902,	4211 Sansom St., Philadelphia.
2468.	SCHIAPARELLI, GIOVANNI	Feb'y 15, 1901,	Royal Observatory, Milan, Italy.
1964.	SCHURZ, CARL, Hon.	Sept. 20, 1878,	54 William St., New York.
1725.	SCLATER, PHILIP LUTLEY, Ph.D.	April 18, 1873,	3 Hanover Square, London, W., England.
2372.	SCOTT, CHARLES F.	Feb'y 18, 1898,	Westinghouse Electric Co., Pittsburgh, Pa.
2112.	SCOTT, WILLIAM B., Prof. . . .	Dec. 17, 1886,	Princeton, N. J.
1870.	SCUDDER, SAMUEL HUBBARD. . .	Sept. 20, 1878,	Cambridge, Mass.
2352.	SEE, THOMAS J. J., LL.D. . . .	Dec. 17, 1897,	Mare Island Observatory, Cal.
1704.	SELLERS, COLEMAN, Sc.D. . . .	July 19, 1872,	3301 Baring St., Philadelphia.
2430.	SELLERS, COLEMAN, JR.	Dec. 15, 1899,	410 N. 33d St., Philadelphia.
1533.	SELLERS, WILLIAM.	April 15, 1864,	1819 Vine St., Philadelphia.
2057.	SERGI, GIUSEPPE, Prof.	Oct. 16, 1885,	Università Romana, Rome, Italy
2076.	SHARP, BENJAMIN, M.D.	May 21, 1886,	Academy of Natural Sciences, Philadelphia.
1960.	SHARPLES, STEPHEN PASCHALL, Prof.	April 21, 1882,	26 Broad St., Boston, Mass.
1797.	SHERWOOD, ANDREW	Oct. 15, 1875,	Mansfield, Tioga Co., Penna.
1822.	SHIELDS, CHAS. W., LL.D., Rev..	Feb'y 2, 1877,	Princeton, N. J.
2442.	SIGSBEE, CHARLES D., Admiral, U.S.N.	Dec. 15, 1899.	League Island Navy Yard, Philadelphia.
2419.	SINKLER, WHARTON, M.D.	May 18, 1900.	1606 Walnut St., Philadelphia
2351.	SMITH, A. DONALDSON, M.D. . .	Oct. 15, 1897.	1820 Chestnut St., Phila.
2146.	SMITH, EDGAR F., Prof.	Oct. 21, 1887,	3421 Walnut St., Philadelphia.
1789.	SMITH, STEPHEN, M.D	Oct. 15, 1875,	57 W. 42d St., New York.
2335.	SMOCK, JOHN C., Prof.	Oct. 15, 1897,	Trenton, N. J.
2141.	SMYTH, ALBERT H., Prof. . .	May 20, 1887,	5214 Main St., Germantown, Philadelphia.
2229	SNELLEN, HERMAN, JR., Ph.D. .	Feb'y 16, 1894,	Utrecht, Netherlands.
1742.	SNOWDEN, A. LOUDON	Oct. 17, 1873,	1812 Spruce St., Philadelphia.
2009.	SNYDER, MONROE B., Prof. . . .	Jan'y 18, 1884,	2402 N. Broad St., Philadelphia.
1720.	SPOFFORD, A. R., LL.D.	Jan'y 17, 1873,	Library of Congress, Washington, D. C.
2502.	STENGEL, ALFRED, M.D.	April 3, 1903,	1811 Spruce St., Philadelphia.
2348.	STEPHENS, H. MORSE, Prof. . . .	Oct. 15, 1897,	University of California, Berkeley, Cal.
1990.	STEVENS, WALTER LECONTE, Prof.	Jan'y 18, 1884,	Lexington, Va.
1840.	STEVENSON, JOHN JAMES, Prof. .	April 20, 1877,	University Heights, New York
2276.	STEVENSON, SARA Y., Sc.D. . . .	Oct. 18, 1895,	237 S. 21st St., Philadelphia.
2371.	STILLWELL, L. B.	Feb'y 18, 1898,	6th St., Lakewood, N. J.
2486.	STONEY, G. JOHNSTONE, Prof., F.R.S.	April 4, 1902.	30 Ledbury Rd., Bayswater, London, W., Eng.

	Name.	*Date of Election.*	*Present Address.*
2094.	SUESS, EDUARD, Prof.	May 21, 1886,	K. K. Geologische Reichsan stalt, Vienna, Austria.
2258.	SULZBERGER, MAYER, Hon. . . .	May 17, 1895,	1303 Girard Ave., Philadelphia.
2092.	SZOMBATHY, JOSEF, Prof.	May 21, 1886,	Burgring 7, Vienna, Austria.

T

2328.	TATHAM, WILLIAM	Oct. 15, 1897,	1811 Walnut St., Philadelphia
2098.	TEMPLE, RICHARD CARNAC, Col. .	May 21, 1886,	Port Blair, Andaman Islands, Bengal, India.
2289.	TESLA, NIKOLA, LL.D.	May 15, 1896,	Wardenclyffe, Long Island, N.Y.
2006.	THOMAS, ALLEN C., Prof.	Jan'y 18, 1884,	Haverford, Pa.
1993.	THOMPSON, HEBER S.	Jan'y 18, 1884,	Sheafer Build'g, Pottsville, Pa.
1726.	THOMPSON, SIR HENRY, Bart. .	April 18, 1873,	35 Wimpole St., Cavendish Square, London, England.
2488.	THOMPSON, SILVANUS P., Prof., F.R.S.	April 4, 1902,	Technical College, Finsbury, Leonard St, City Rd., E. C., Eng.
1807.	THOMSON, ELIHU, Prof.	April 21, 1876,	Swampscott, Mass.
2507.	THOMSON, JOSEPH JOHN, D.Sc., F.R.S	April 3, 1903,	TrinityCollege, Cambridge, Eng.
1909.	THOMSON, WILLIAM, M.D.	April 16, 1880,	1426 Walnut St., Philadelphia
2052.	IM THURN, EVERARD F.	Oct. 16, 1885,	Queens College, Neawara Eliya, Ceylon.
1530.	THURY, A., Prof.	April 15, 1864,	Univ. of Geneva, Geneva, Switzerland.
2471.	TILGHMAN, BENJAMIN CHEW . .	April 4, 1902,	1126 S. 11th St., Philadelphia.
2123.	TOPINARD, PAUL, Prof.	Dec. 17, 1886,	105 Rue de Rennes, Paris, France.
2249.	TOWER, CHARLEMAGNE, JR., LL.D., Hon.	Feb'y 15, 1895,	U. S. Embassy, Berlin, Gemany.
2503.	TRELEASE, WILLIAM, Sc.D	April 3, 1903,	Missouri Botanical Garden, St. Louis, Mo.
2413.	TREVELYAN, GEORGE OTTO, Rt. Hon. Sir	May 19, 1899,	8 Grosvenor Crescent, S. W. London, England.
2288.	TROWBRIDGE, JOHN, Prof.	May 15, 1896,	Harv. Univ., Cambridge, Mass.
2441.	TRUE, FREDERICK WILLIAM, Dr .	Dec. 15, 1899,	U. S. National Museum, Washington, D. C.
1973.	TSCHERMAK, GUSTAV.	Oct. 20, 1882,	Universität, Vienna, Austria.
2321.	TSCHERNYSCHEW, THEODORE, Prof.	May 21, 1897,	Geological Survey, St. Petersburg, Russia.
1529.	V. TUNNER, PETER R., Prof. . . .	April 15, 1864,	Leoben, Austria.
1983.	TURRETTINI, THEODORE, Prof . .	Dec. 19, 1890,	Geneva, Switzerland.
2166.	TUTTLE, DAVID K., Ph.D.	Oct. 18, 1889,	U. S. Mint, Philadelphia.
2163.	TYLER, LYON G., Hon., Pres't . .	Oct. 18, 1889,	Williamsburg, Va.
2138.	TYSON, JAMES, M.D	May 20, 1887,	1506 Spruce St., Philadelphia.

U

2185.	UNWIN, WILLIAM C., Prof.	Dec. 19, 1890,	7 Palace Gate Mansions, London, England.

V

2400.	VAUCLAIN, SAMUEL M.	May 19, 1899,	1533 Green St., Philadelphia.
2325.	VAUX, GEORGE, JR.	Oct. 15, 1897,	404 Girard Building, Phila.
1670.	VOSE, GEORGE L., Prof.	Oct. 21, 1870,	Brunswick, Maine.

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2186.	VOSSION, LOUIS	Dec. 19, 1890,	Consulate of France, Bombay, India.
2508.	DE VRIES, HUGO, Prof.	April 3, 1903,	University of Amesterdam, Amsterdam, Netherlands.

W

2034.	WAGNER, SAMUEL	Jan'y 16, 1885,	Greenbank Farm, West Chester, Pa.
1748.	WAHL, WILLIAM H., Ph.D. . . .	Jan'y 16, 1874,	15 S. 7th St., Philadelphia.
2331.	WALCOTT, CHARLES D., LL.D. .	Oct. 15, 1897,	U. S. Geological Survey, Washington, D. C.
1724.	WALLACE, ALFRED RUSSEL, LL.D.	April 18, 1873,	Parkstone, Dorset, England.
2156.	WARD, LESTER F., LL.D.	May 17, 1889,	1464 Rhode Island Ave., Washington, D. C.
1925.	WARE, LEWIS S	Jan'y 21, 1881,	Phila.BookCo.,15 S.9th St.,Phila.
2359.	WARFIELD, ETHELBERT D., Pres't	Dec. 17, 1897,	Easton, Pennsylvania.
2033.	WEIL, EDWARD HENRY	Jan'y 16, 1885,	1720 Pine St., Philadelphia.
2286.	WELCH, WILLIAM H., M.D. . . .	May 15, 1896,	935 St. Paul St., Baltimore, Md.
1639.	WHARTON, JOSEPH.	April 16, 1869,	P. O. Box 1332, Philadelphia.
1637.	WHITE, ANDREW D., Hon.	April 16, 1869,	White Library, Cornell Univ., Ithaca, N. Y.
1848.	WHITE, ISRAEL C., Prof.	Jan'y 18, 1878,	119 Wiley St., Morgantown, W. Va.
2384.	WHITFIELD, R. P., Prof. . . .	May 20, 1898,	American Museum of Natural History, New York.
2439.	WHITMAN, CHARLES OTIS, Prof. .	Dec. 15, 1899,	University of Chicago, Chicago, Ill.
1863.	WILDER, BURT G., Prof.	May 3, 1878,	60 Cascadilla Pl., Ithaca, N. Y.
2250.	WILLCOX, JOSEPH	Feb. 15, 1895,	"The Clinton," 10th and Clinton Sts., Philadelphia.
2347.	WILLIAMS, EDWARD H., JR., Prof.	Oct. 15, 1897,	53 Phillips St., Andover, Mass.
2151.	WILLIAMS, TALCOTT, LL.D. . . .	May 18, 1888,	916 Pine Street, Philadelphia.
2178.	WILLIS, HENRY, Prof.	Feb'y 21, 1890,	4036 Baring St., Philadelphia.
2041.	WILSON, JAMES CORNELIUS, M.D.	Jan'y 16, 1885,	1511 Walnut St., Philadelphia.
2137.	WILSON, WILLIAM POWELL, M.D..	May 20, 1887,	233 S. 4th St., Philadelphia.
2341.	WILSON, WOODROW, Pres't. . . .	Oct. 15, 1897,	Prospect, Princeton, N. J.
2216.	WISTAR, GEN. ISAAC J.	May 19, 1893,	S. E. Cor. Spruce and 17th Sts., Philadelphia.
2314.	WISTER, OWEN	May 21, 1897,	328 Chestnut Street, Phila.
2343.	WITMER, LIGHTNER, Ph.D., Prof.	Oct. 15, 1897,	University of Penna , Phila.
1884.	WOOD, RICHARD.	April 18, 1879,	1620 Locust St , Philadelphia.
2408.	WOOD, STUART.	May 19, 1899,	1620 Locust St., Philadelphia.
1762.	WOODWARD, HENRY, LL D , F.R.S.	July 17, 1874,	British Museum, London, England.
2478.	WOODWARD, ROBERT S., Ph.D., Prof.	April 4, 1902,	408 West 145th St., New York.
2290.	WRIGHT, ARTHUR W., Ph.D., Prof	May 15, 1896,	73 York Sq., New Haven, Conn.
2448.	WRIGHT, WILLIAM ALDIS, LL.D .	Feb'y 16, 1900,	Trinity College, Cambridge, England.
2214.	WUNDT, WILLIAM, Prof.	Feb. 15, 1895,	Leipzig, Germany.
2426.	WURTS, ALEXANDER JAY	Dec. 15, 1899,	Nernst Lamp Co., Pittsburg, Pa.
1932.	WURTS, CHARLES STEWART, M.D.	Jan'y 21, 1881,	1701 Walnut St., Philadelphia.
2061.	WYCKOFF, A. B., Lieut. U. S. N. .	Feb'y 19, 1886,	Navy Department, Washington, D. C.

Y

1904.	YARNALL, ELLIS	April 16, 1880,	420 Walnut St., Philadelphia.
1769.	YOUNG, CHARLES AUGUSTUS, Prof.	April 17, 1874,	16 Prospect Av., Princeton, N. J.

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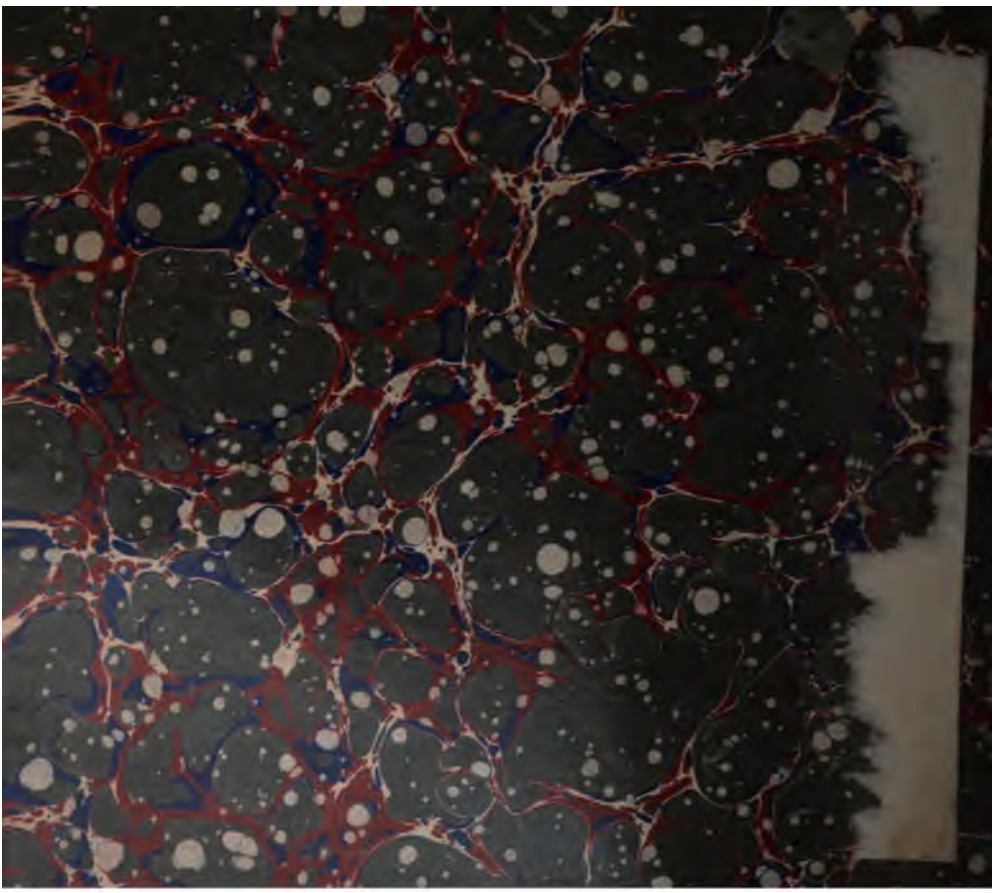
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